Experiments on cooperation, institutions, and social preferences

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Contents

List of Figures

List of Tables

Chapter 1

Introduction

This PhD dissertation consists of three chapters in experimental economics. It involves various dimensions in which laboratory experiments can play a role: testing the validity of a game theory, helping understand institutions, and measuring (the change in) social preferences. It relates to the effects of different institutions on cooperation and social preferences. Chapter 2 studies to what extent an overlapping membership structure, which in theory affects the incentives of short-lived players, is conducive to cooperation. Chapter 3 examines whether the presence of decentralized punishment, especially the possibility of retaliating a centralized enforcer, has an impact on the decisions of the enforcer and group cooperation. Chapter 4 studies whether interactions with out-group members matter for in-group-out-group differences in altruism and whether the nature of these interactions matters for in-group-out-group differences.

Chapter 2 is about overlapping membership structures within an organization. Cremer (1986), Salant (1991), Kandori (1992a), and Smith (1992) theoretically prove that cooperation is possible to be sustained as an equilibrium outcome in repeated social dilemma games with short-lived members of an organization (players), if the organization (the replacement of players) is *ongoing*. The key condition is that members are not all replaced by new members at the same time, that is, memberships in the organization are *overlapping*. In Chapter 2, the theory is put to the test. An experiment is set up in which there are multiple ongoing organizations for an indefinite number of rounds. In each round, there are two players (subjects) in each organization and they play a prisoner's dilemma game. Organizations with an overlapping membership structure are compared to organizations with a non-overlapping membership structure.

The experimental results show that there is at best weak evidence that an overlapping membership structure induces a higher cooperation rate. On the other hand, subjects' behavioral patterns are affected by the overlapping membership structure. Junior members behave more cooperatively than senior members do in organizations with an overlapping membership structure. Besides, incoming members are more sensitive to organizational history in organizations with an overlapping membership structure than in those with a non-overlapping membership structure. This chapter contributes to the literature by shedding light on how difficult it is to sustain cooperation in the prisoner's dilemma when subjects play finitely repeated games. It may also constitute an important part of our understanding of organizational culture by demonstrating that cooperative incentives can be transmitted from one generation to the next.

Chapter 3 is a study on the interaction between institutions and its impact on cooperation. Previous literature has shown that when the sanctioning power is delegated to *a legitimate enforcer*, free riding can be effectively deterred in social dilemmas so that high cooperation can be induced. Chapter 3 examines when there is a possibility of *decentralized punishment*, especially a possibility of retaliating the enforcer, whether and how the enforcer's sanctioning decisions and group cooperation will change. In particular, I look at both corruptible and non-corruptible enforcers. I set up an experiment with a 2×2 design, varying whether there is a possibility of decentralized punishment and whether the enforcer is corruptible. In the experiment, there are groups of four members and they play a public goods game. Afterwards, the enforcer who was randomly selected redistributes earnings among members, and in some conditions then all members can reduce any other member's earnings with a cost.

The experimental results demonstrate different effects of the decentralized punishment possibility. On the one hand, for a non-corruptible enforcer, her centralized sanctions are reduced by the possibility of being retaliated. But group contributions are not lowered correspondingly, since peer punishments on free riders offset the decrease in pro-social centralized sanctions. On the other hand, for a corruptible enforcer, even though her excessive and corruptive sanctions are not really restrained by the presence of decentralized punishment, group contributions decrease as centralized sanctions become less pro-social. Chapter 3 contributes to the literature by revealing that dismissing the possibility of decentralized punishment may lead to an overestimation of the effectiveness of centralized sanctioning institutions in improving cooperation. Besides, it extends the study about the impact on decentralized punishment of second-order decentralized punishment to the impact on centralized punishment.

Chapter 4 studies the impacts of competitive and cooperative interactions with out-group members on in-group-out-group differences in social preferences. Suppose that people of different races competed for a job position, will they treat each other in a more hostile way? Or if they all donated to the same charity, will they feel more favorably towards each other? To answer these questions, An experiment is set up in which subjects are randomly assigned to either the Red group or the Blue group. They are asked to do a task which generates earnings and the nature of interactions is manipulated with different ways of calculating earnings, either cooperative or competitive. There is also a baseline condition in which subjects' earnings are calculated in a piece rate. After the task, without knowing its outcome, subjects are asked to make distributional decisions between self and another randomly selected participant, either from their own group (in-group matching) or the other group (out-group matching). In-group-out-group differences are captured by the differences in their choices between in-group matching and out-group matching (Chen and Li 2009; Currarini and Mengel 2016).

The experimental results show that when subjects receive a higher payoff than their matched players do, cooperative interactions with out-group members decrease the in-group-out-group difference in altruism, but competitive interactions do not have an impact. This chapter extends studies on the effect of "personal" contact on out-group prejudice to an environment with "impersonal" interaction that only an abstract economic interdependence is imposed between persons from different groups. It also contributes to relatively scarce literature measuring in-group-out-group differences in terms of social preferences.

Chapter 2

An Experiment on Cooperation in Ongoing Organizations

2.1 Introduction

In an important paper, Cremer (1986) shows that cooperation among the members of an organization is possible, even if members have finite lives, as long as the organization itself is ongoing. The key condition is that members are not all replaced by new members at the same time. If members share a common last round, the standard backward induction argument of unraveling of cooperation applies. If, however, membership is overlapping (staggered) there is no common last round. There is always a member whose horizon extends beyond that round, and who needs to take into account the strategy of a new incoming member. If this strategy involves a reward for cooperative behavior, cooperation can be sustained as an equilibrium outcome.

The model of Cremer (1986) is an application of the overlapping generations model introduced by Samuelson (1958). Its relevance extends beyond cooperation in organizations. Other models analyze, for instance, the sustainability of pay-as-you-go pension plans (Hammond 1975), the supply of intergenerational club goods (Sandler 1982), the scope for arms control between countries (John *et al.* 1993), the interaction between junior and senior members of a political party (Alesina and Spear 1988), and the collaboration between regulatory agents and firm managers (Salant 1995). Several studies indicate that the scope for cooperation between finitely-lived players is furthered by the condition that life spans and terms overlap rather than fully coincide (Salant 1991; Kandori 1992a; Smith 1992).

In the present paper we put this argument to the test. We set up a laboratory experiment in which an organization exists for an indefinite number of rounds. In each round, an organization is inhabited by two members who play a prisoner's dilemma game. The two members interact with each other for a fixed number of rounds (either one or three rounds). We implement two different term structures: an overlapping (OL) structure in which the two members are replaced by new members in different rounds, and a non-overlapping (NoOL) structure in which the two members are replaced in the same round. In line with the analysis of Cremer (1986), we hypothesize that the average cooperation rate will be higher in organizations with an overlapping structure than in those with a non-overlapping structure.

The experimental results show at best weak support for our main hypothesis. Cooperation rates are not significantly different between organizations with an overlapping membership structure and those with a non-overlapping structure. Moreover, this holds for the case in which members overlap for one round and the case in which they overlap for three rounds. This does not imply that play is completely insensitive to the overlapping membership structure. We find that junior (incoming) members cooperate at a higher rate than senior (outgoing) members. Also, junior members cooperate at a higher rate when the senior member they interact with cooperated in the previous round. Such strategic play is not strong enough though to induce substantially higher rates of cooperation.

There are a few related experimental studies on cooperation in games with an overlapping generations structure. Van der Heijden *et al.* (1998) examine whether the provision of information feedback on the history of play has an effect on the level of inter-generational transfers. It turns out that it does not have an effect, suggesting that players do not use this information in a strategic way. Offerman *et al.* (2001) use the strategy method to study play in an inter-generational prisoner's dilemma game. They find that relatively few subjects use history-dependent strategies, such as trigger strategies, even when recommended to do so by the experimenters. A recent study by Duffy and Lafky (2016) has a focus similar to ours. It compares contributions in public goods games with and without an overlapping generations structure. They find that average contribution levels are not affected by the matching structure, but that the pattern of contributions over time is more stable with overlapping matches.

2.2 Theoretical Framework

Our theoretical framework is based on models that study the scope for cooperation in games with an overlapping membership structure (Cremer 1986; Salant 1991; Kandori 1992a; Smith 1992). It involves an organization that lasts for an indefinite number of rounds. In each round there are two members (players) in the organization. One member is assigned role A and the other is assigned role B. The two members play a symmetric prisoner's dilemma (PD) game as displayed in Table 2.1.

Table 2.1: Prisoner's dilemma game

		B: C	B: D
A	C	(2,2)	(0,3)
A	D	(3,0)	(1,1)

The membership of the organization changes over time. Let i_τ denote the member of role i coming to the organization in round τ, where $i \in \{A, B\}$ and $\tau \in \{0, 1, 2, 3, ...\}$. Except for A_0 who is only active for one round, each member stays in the organization for two rounds. Once a member finishes her membership in the organization, she is replaced by an incoming member of the same role. In each round, one member in the organization is replaced. Hence, the membership of each member overlaps the membership of one other member for one round. This matching structure with 2-round memberships and 1-round overlapping memberships is depicted in Table 2.2.

Table 2.2: Players with 2-round memberships and 1-round overlapping memberships

	Round					
Role	1	2	3	4	5	...
A	A_0	A_2	A_2	A_4	A_4	...
B	B_1	B_1	B_3	B_3	B_5	...

If the PD game is played repeatedly by finitely lived players with overlapping memberships, there exist subgame perfect equilibria with cooperative outcomes. In a player's last round in the organization, it is always optimal to defect since there is no shadow of the future. Cooperative incentives can only emerge before players are in the last round of their membership. Label players in their first (last) round in the organization as junior (senior). Consider the strategy profile in which players cooperate if and only if they are juniors and they see that all preceding members cooperated when they were juniors. It is not profitable to deviate from cooperation to defection when a junior faces a history in which there was no defection. If a junior cooperates, she will elicit cooperation when she is a senior, assuming an incoming member sticks to the equilibrium strategy. Her total equilibrium payoff is $\pi(C, D) + \pi(D, C) = 3$. If a junior player defects, she will face defection when she is senior, which yields total payoff $\pi(D, D)+\pi(D, D) = 2$. The overlapping membership structure allows for partial cooperation, with an increase in average per-round payoffs from 1 to 1.5, compared with the equilibrium in which players always defect.

Let $x^s_{i,\tau}$ denote the action of player i_τ in term s, where $s \in \{1, 2\}$. Let $\Delta_{i,\tau} = (\Delta^1_{i,\tau}, \Delta^2_{i,\tau})$ denote the strategy profile of player i_τ. Specifically, $\Delta^s_{i,\tau}$ stands for the probability that player i_τ in term s plays C. A subgame perfect equilibrium strategy profile is the following.[1]

$$\Delta^s_{i,\tau} = \begin{cases} 1 & \text{if } s = 1 \text{ and } x^1_{j,t} = C \ for\ all\ j \in \{A, B\},\ for\ all\ t < \tau \\ 0 & \text{otherwise} \end{cases}$$

The possibility for cooperative equilibria extends to games with longer memberships. In particular, we consider a game in which a member stays in the organization

[1]Note that this grim trigger strategy is not the only strategy that can sustain cooperation. For example, there is a "resilient" strategy that punishes defectors, but does not punish punishers, which can also sustain cooperation by junior members as a subgame perfect equilibrium (Bhaskar 1998).

for six rounds, except for A_0 who is in the organization for only three rounds. Once a member finishes her membership in the organization, she is replaced by an incoming member of the same role. One member in the organization is replaced every three rounds. Hence, the membership of each member overlaps the membership of one other member for three rounds. This matching structure with 6-round memberships and 3-round overlapping memberships is depicted in Table 2.3. Again, let i_τ denote the player with role i entering the organization in round τ.

Table 2.3: Players with 6-round memberships and 3-round overlapping memberships

	Round													
Role	1	2	3	4	5	6	7	8	9	10	11	12	13	...
A	A_0	A_0	A_0	A_4	A_4	A_4	A_4	A_4	A_4	A_{10}	A_{10}	A_{10}	A_{10}	...
B	B_1	B_1	B_1	B_1	B_1	B_1	B_7	B_7	B_7	B_7	B_7	B_7	B_{13}	...

It is still optimal for players to defect in the last round of their terms. The most efficient equilibrium outcome can be sustained as follows. Consider a strategy profile in which players cooperate if and only if they are in one of their first five rounds in the organization and they see that all preceding members cooperated in their first five rounds in the organization.

Except that $s \in \{1, 2, ..., 6\}$, notations for this case are the same as before. A cooperative subgame perfect equilibrium strategy profile is as follows.

$$\Delta^s_{i,\tau} = \begin{cases} 1 & \text{if } s \leq 5 \text{ and } x^k_{j,t} = C \ for\ all\ j \in \{A, B\},\ for\ all\ t < \tau,\ for\ all\ k \leq 5 \\ 0 & \text{otherwise} \end{cases}$$

It is easily checked that this strategy profile constitutes a subgame perfect equilibrium. Compared to the equilibrium in which all players always defect, the cooperative equilibrium increases average per-round payoff from 1 to 1.83. In the experiment we aim to explore to what extent this cooperative potential is realized.

In contrast, if players of both roles enter and exit the organization at the same time, there exists a unique uncooperative subgame perfect equilibrium in which all players always defect (zero cooperation rate), regardless of finite lengths of memberships.

If we take into account social preferences, such as referring to Fehr and Schmidt (1999), cooperative equilibria are possible to be sustained even if memberships are *not* overlapping. Take the case of 1-round non-overlapping memberships as an example. If $\beta \geq 1/3$, where β captures advantageous inequality aversion, full cooperation can be sustained as an equilibrium outcome.[2] Then cooperation rates are expected to be *high* with both overlapping and non-overlapping membership structures.

[2]The condition is derived from $u(C, C) = 2 \geq u(D, C) = 3 - \beta(3 - 0)$. But to cooperate is not a dominant strategy since $u(D, D) = 1 > u(C, D) = 0 - \alpha(3 - 0)$ always holds, where $\alpha > 0$ captures disadvantageous inequality aversion.

2.3 Experimental Design, Hypotheses and Procedure

2.3.1 Design

In all the sessions of our experiment, subjects repeatedly play the PD game displayed in Table 2.1. Since infinite repetitions cannot be implemented in the lab, a random continuation rule is employed. Each session consists of at least 30 rounds. Starting from the 30th round, after a round finishes, the computer randomly draws a number between 1 and 100. If the number is smaller than or equal to 90, the experiment continues for one more round; if the number is larger than 90, the experiment stops. The probability that the experiment continues for at least one more round after the 30th round is 90%.[3]

There are four treatments in our experiment, 1-OL, 1-NoOL, 3-OL, and 3-NoOL. In all treatments, there are multiple organizations. Each organization has two members (subjects) in each round. After a subject finishes her membership in one organization, she switches to a new organization which is randomly selected.

The treatments differ in matching protocols. In the 1-OL treatment, the membership of each organization changes as displayed in Table 2.2; in each round one of the two members is replaced by a new member. In the 1-NoOL treatment, membership of the organization also changes from one round to the next, but now both members are replaced at the same time. What is common is that in both the 1-OL treatment and the 1-NoOL treatment members interact with a different member after each round.

In the 3-OL treatment, the membership of the organization changes as displayed in Table 2.3; after every 3 rounds one of the two members is replaced by a new member. In the 3-NoOL treatment, membership of the organization also changes after every 3 rounds, but now both members are replaced at the same time. What is common is that in both the 3-OL treatment and the 3-NoOL treatment members interact with a different member after three rounds.

In our design, the number of rounds a member interacts with the same other member (either 1 or 3) is kept constant between the OL and NoOL treatments. This implies that the number of rounds a member is in an organization is larger in the OL treatments (2 or 6 rounds) than in the NoOL treatments (1 or 3 rounds).

One feature of our experiment is that re-matching across organizations is allowed.

[3]There are basically four approaches to implement infinite repetitions with discounting: random continuation rule (RT), fixed part with payoff discounting plus random continuation rule (D+RT), fixed part with payoff discounting plus coordination game (D+C), and block random continuation rule (BRT). These approaches are discussed in Frechette and Yuksel (2013). We use a variation of the second approach (D+RT) with a fixed part without payoff discounting (see also Norman and Wallace 2012). This approach implements a degree of discounting to sustain cooperative equilibria ($0.9 < \delta < 1$) and guarantees there is a minimum number of rounds before the game ends.

Besides being practical, re-matching is not unrealistic in organizational contexts and captures features of job rotation and turnover.[4] Possible effects of re-matching on results are explored and discussed in section 2.4.4.

In all treatments, subjects have access to the complete decision history of their current organization. At the end of each round, they are also informed of their own earnings and the earnings of the member they just interacted with.

2.3.2 Hypotheses

The first prediction is that there is a difference between the cooperation rates of the OL and NoOL treatments. For the 1-OL treatment, the most efficient subgame perfect equilibrium entails an average cooperation rate of 50%. For the 3-OL treatment, the most efficient equilibrium involves an average cooperation rate of 83.3%. There is no cooperative subgame perfect equilibrium for the 1-NoOL and 3-NoOL treatments so that the average cooperation rates of the 1-OL and 3-NoOL treatments are hypothesized to be zero.[5]

H1.a: The cooperation rate in the 1-OL treatment is higher than that in the 1-NoOL treatment.

H1.b: The cooperation rate in the 3-OL treatment is higher than that in the 3-NoOL treatment.

The second prediction is about subjects' junior and senior terms in the organization. For a subject in the 1-OL treatment, term 1 is her junior term and term 2 is her senior term. For the 3-OL treatment, it is less obvious to define junior and senior terms. Since the most efficient equilibrium outcome indicates that there is reduction in cooperative incentive from term 5 to term 6, we define a subject's junior terms as consisting of her first five terms and her senior terms as consisting of her last term. According to the equilibrium strategies discussed above, subjects in the OL treatments behave more cooperatively in their junior terms than in their senior terms.

H2.a: The cooperation rate over subjects' junior terms is higher than that over their senior terms in the 1-OL treatment.

H2.b: The cooperation rate over subjects' junior terms is higher than that over their senior terms in the 3-OL treatment.

[4]Otherwise, we have to recruit more subjects for each session and let them wait before they are assigned to an organization and after they leave an organization.

[5]Kandori (1992b) extends the Folk theorem of repeated fixed matching games to random matching games, by referring to "contagious equilibrium". But this theoretical possibility is not empirically supported. Duffy and Ochs (2009) find that no cooperative norm emerges in random matching games which theoretically sustain cooperation. We hereby choose to stick to the equilibrium in which players always defect when they are randomly (re)matched.

The final set of hypotheses concern organizational history. Even though organizational history is displayed in all treatments, its strategic relevance varies across matching protocols. If a subject switches to a new organization, in the OL treatments she sees the previous decision(s) of the other active member she will interact with in the new organization; while in the NoOL treatments she is only exposed to previous decision(s) of members who have already left the current organization. Incoming members in the OL treatments can punish or reward the other active (senior) member based on the organizational history, but in the NoOL treatments an incoming member does not obtain any strategically relevant information. This difference in strategic relevance of organizational history motivates the following hypotheses.

H3.a: Incoming members in the 1-OL treatment are more sensitive to organizational history than those in the 1-NoOL treatment.

H3.b: Incoming members in the 3-OL treatment are more sensitive to organizational history than those in the 3-NoOL treatment.

2.3.3 Procedure

The experiment was run in March and April, 2015 at Centerlab, Tilburg University and it was computerized using the Z-tree software (Fischbacher 2007). Subjects were Tilburg students and recruited via an online system. Upon arrival, subjects were assigned to computers by randomly choosing one card from a pile of numbered cards. Since each session required an even number of participants, some students who showed up could not participate but got a show-up fee. Once subjects were seated in the lab, printed copies of the instructions were distributed and subjects got ample time to read the instructions and ask questions. After they answered all control questions, the experiment started. When the experiment ended, two rounds were randomly chosen and earnings in the two rounds were added up for subjects' final earnings.

In total, 16 sessions were run and 228 subjects participated in the experiment. Each treatment consisted of 4 sessions. The number of subjects in each session ranged from 12 to 18 and the number of organizations ranged from 6 to 9. In each session, the organizations were divided into two independent matching groups, except for sessions 8 and 11 which had only one matching group because too few people showed up. On average, each session lasted for 38 rounds and took about 45 minutes. Subjects earned 9.4 euro on average, with a minimum of 2.5 euro and a maximum of 20.5 euro.

2.4 Experimental Results

2.4.1 Cooperation across Treatments and over Time

Table 2.4 displays average cooperation rates by treatment as well as rank-sum tests comparing the treatments.

Table 2.4: Average cooperation rates by treatment

Treatment	OL	NoOL	p-value	row total
1-round	13.47 (7.99) N = 8	12.61 (6.85) N = 7	0.82	13.07 (7.23) N = 15
3-round	19.82 (12.40) N = 7	17.37 (15.27) N = 8	0.56	18.51 (13.57) N = 15
p-value	0.24	0.91		0.37
column total	16.43 (10.42)	15.15 (11.95)	0.52	15.79 (11.04)

Notes: Cooperation rates are reported in percentages. An independent matching group is a unit of observation. Standard deviations are in parentheses. N denotes the number of independent matching groups.

The average cooperation rate across all the treatments is 15.79%. The average cooperation rate in the 1-OL treatment is 13.47%, which is higher than that in the 1-NoOL treatment (12.61%). Also, the average cooperation rate in the 3-OL treatment (19.82%) is higher than that in the 3-NoOL treatment (17.37%).

We conduct rank-sum tests on cooperation rates, using matching groups as units of independent observations. The cooperation rates in the 1-OL and 1-NoOL treatments are not significantly different (p-value = 0.82). The same holds for the cooperation rates in the 3-OL and 3-NoOL treatments (p-value = 0.56) and those in pooled OL and NoOL treatments (p-value = 0.52).[6] These experimental results do not support the hypothesis that an overlapping membership structure is conducive to cooperation.

We also look at the heterogeneity of cooperation across organizations. According to the theoretical framework in section 2.2, for organizations with an overlapping membership structure there exist multiple equilibria with different cooperation levels; while for organizations with a non-overlapping membership there is a unique non-cooperative equilibrium. Therefore, a natural hypothesis is that the heterogeneity of cooperation across organizations is larger with an overlapping membership structure than with a non-overlapping membership structure.

[6] The cooperation rates in the 1-NoOL and 3-OL treatments are not significantly different either (p-value = 0.22).

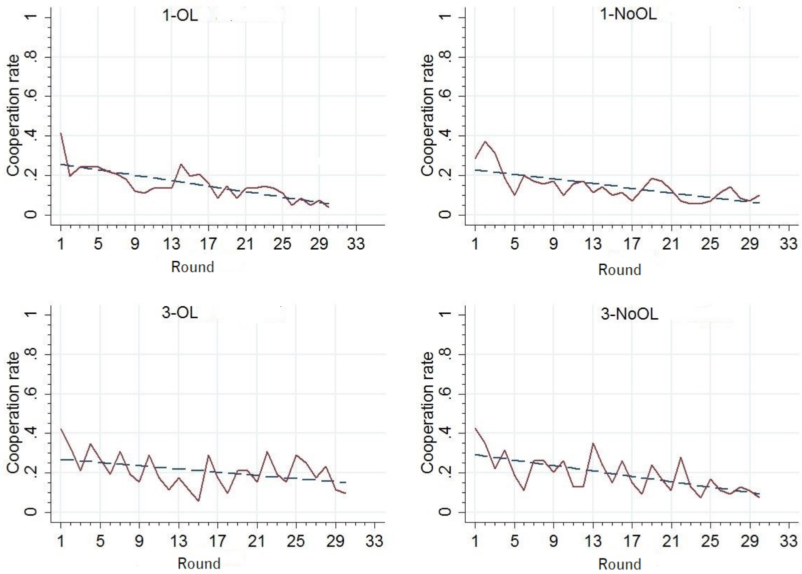
To examine heterogeneity we first calculate the cooperation rate for each organization. We then compute the standard deviation of organizations' cooperation rates within each matching group. This gives us one measure of heterogeneity for each matching group. The averages of these measures by treatment are presented in Table 2.5. We find that the heterogeneity across organizations in the 3-OL treatment is larger than that in the 3-NoOL treatment (significant at 10% with a one-tailed test). Heterogeneity is also somewhat larger in the 1-OL treatment than in the 1-NoOL treatment, but this effect is weaker (p-value = 0.46 with a one-tailed test). So there is limited evidence to support the hypothesis that an overlapping membership structure leads to larger heterogeneity of cooperation across organizations.

Table 2.5: Heterogeneity of cooperation across organizations by treatment

Treatment	OL	NoOL	p-value
	0.05	0.04	
1-round	(0.03)	(0.02)	0.91
	N = 8	N = 7	
	0.09	0.04	
3-round	(0.07)	(0.02)	0.20
	N = 7	N = 8	

Notes: Figures reported in the second and third columns are the averages of the measures of heterogeneity in cooperation rates across organizations by treatment. Standard deviations of these measures are in parentheses. N denotes the number of independent matching groups.

Next, we investigate how cooperation develops over time. Figure 2.1 shows that there is a declining trend of cooperation rates in all treatments. As subjects gain experience they behave less cooperatively on average. No salient differences are found between the OL treatments and the NoOL treatments, in either the level or the declining pattern of cooperation rates. The patterns of the 1-OL/1-NoOL treatment on the one hand and the 3-OL/3-NoOL treatment on the other hand are somewhat different. In the 3-OL and 3-NoOL treatments, cooperation rates display more regular fluctuations over time. The pattern is in line with the fact that subjects are rematched every 3 rounds.



This figure shows how cooperation rates evolve over time. The solid lines are for the raw data and the dashed lines are for fitted values. For most organizations there were more rounds, but for ease of comparison the development is truncated at round 30.

Figure 2.1: Cooperation rates over time

The average cooperation rates over different subsets of rounds are displayed in Table 2.6. We distinguish rounds 1-15, rounds 16-30, and rounds 31 and higher. Recall that all organizations lasted for 30 rounds after which there was a continuation probability of 90%.

Table 2.6: Cooperation rates over round subsets

Treatment	rounds 1-15	p-value	rounds 16-30	p-value	rounds 31-end
1-OL	21.48	0.02	13.44	0.12	7.14
1-NoOL	17.02	0.02	9.90	0.35	7.66
3-OL	23.15	0.24	19.48	0.35	9.75
3-NoOL	23.47	0.02	13.99	0.35	13.27

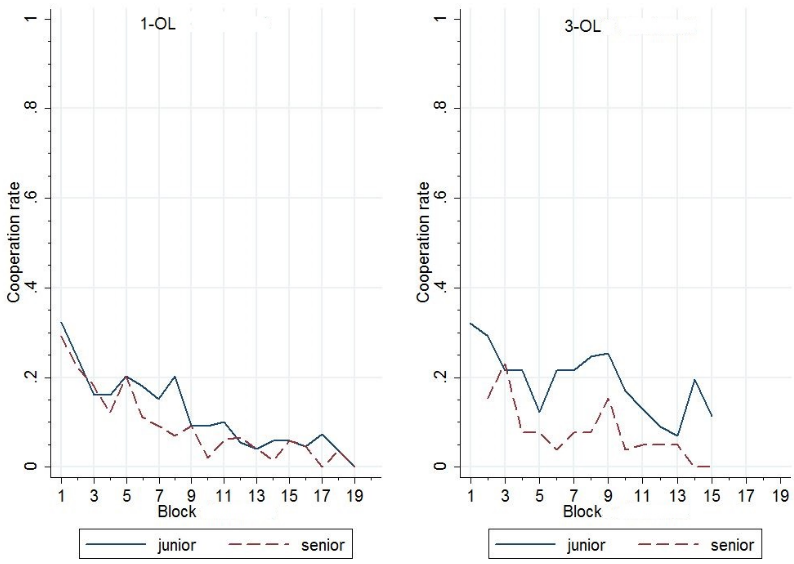
Notes: Cooperation rates are reported in percentages. Independent matching groups are the units of observations. P-values refer to matched-pairs signed-rank tests. The p-values in the 3rd column are for the comparisons between rounds 1-15 and rounds 16-30. The p-values in the 5th column are for the comparisons between rounds 16-30 and rounds 31 and later.

The signed-rank tests show that the cooperation rate over rounds 1-15 is significantly higher than that over rounds 16-30, except in the 3-OL treatment. The significance does not hold for the comparison between the cooperation rates over rounds 16-30 and rounds 31-end. Cooperation decays significantly over the first 30 rounds but not further after round 30. This is not surprising since cooperation rates in some organizations already approach zero by round 30. For no subset of rounds, is there a significant difference between the cooperation rates in the OL and NoOL treatments. These evidences further confirm that an overlapping membership structure is not strongly conducive to cooperation.

To explore behaviors before any re-assignment took place, we also test the treatment effects of overlapping memberships on cooperation rates in round 1 (for both the 1-round and 3-round treatments) and round 3 (only for the 3-round treatments) respectively, excluding the subjects who played for fewer rounds at the start of the experiment. The difference in the cooperation rates in round 1 between the 1-OL and 1-NoOL treatments is marginally significant (p-value $= 0.1$). The cooperation rates are not significantly different between the 3-OL and 3-NoOL treatments in both rounds 1 and 3 (p-value $= 0.5$ for round 1; p-value $= 0.4$ for round 3). These results suggest at best weak differences in cooperation rates between the OL and NoOL treatments before re-assignment.

2.4.2 Junior and Senior Terms

Cooperation rates by subjects' junior and senior terms are presented in Figure 2.2. We see that the cooperation rates are lower over subjects' senior terms than over their junior terms.



The graphs display the cooperation rates of juniors and seniors over time in the 1-OL and 3-OL treatments. The horizontal axis denotes block, which consists of three consecutive rounds. For example, rounds 1-3 constitute Block 1, rounds 4-6 constitute Block 2, and so on.

Figure 2.2: Cooperation rates of juniors and seniors over time

Table 2.7: Cooperation rates by junior and senior terms

	Junior	Senior	p-value
1-OL	15.06	11.94	0.09
3-OL	20.97	9.47	0.03

Notes: Cooperation rates are reported in percentages. Column 4 displays the p-values of two-sided signed-rank tests comparing subjects' senior and junior terms.

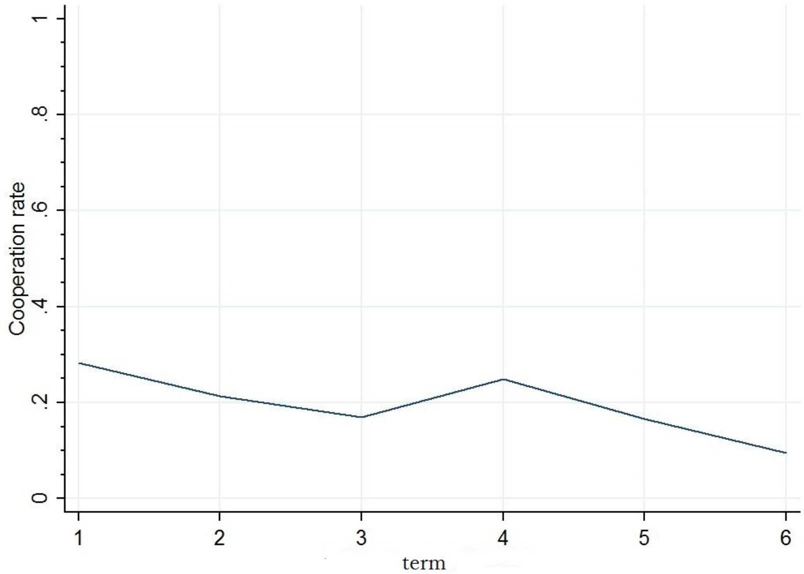
The cooperation rates over subjects' junior terms are significantly higher than those over their senior terms in both the 1-OL and 3-OL treatments (at 5% with a one-tailed test). This outcome supports Hypothesis 2 that the juniors are more cooperative than the seniors.[7]

One may wonder whether the lower cooperation rates of senior members are due to a general declining trend of cooperation (see Figure 2.1). After all, on average senior members act in later rounds than junior members do. Indeed, estimation results reveal a significantly negative effect of the round number on cooperation. However, even if we control for this effect, we still find a significantly positive effect of junior membership on cooperation rates. This holds for both the 1-OL and 3-OL treatments. Results are reported in Table 2.12 in Appendix.[8] These results indicate that junior members cooperate at a higher rate than senior members do even when the negative time trend is controlled for.

For the 3-OL treatment, we also explore how cooperation develops over subjects' six terms in the organization. The results are in Figure 2.3.

[7] This result is robust to other definitions of junior term in the 3-OL treatment (consisting of first 1, 2, 3, and 4 terms) (p-value ≤ 0.06).

[8] The positive effect of being a junior with the parametric test does not depend much on how we define the junior term in the 3-OL treatment. For example, it holds when we define the junior term as the first term in the organization but also when we define it as the first 5 terms in the organization.



This figure shows the cooperation rates over terms in the 3-OL treatment.

Figure 2.3: Cooperation rates over terms in the organization

Subjects behave less cooperatively when they proceed from term 1 to term 3. When they interact with another incoming member in term 4, the cooperation rate increases (p-value=0.09). Subjects also behave less cooperatively from term 4 to term 6. The cooperation rates are lower in terms 3 and 6 than in other terms. In term 6, subjects have no future with their current opponent *and* they are going to leave their current organization. So the cooperation rate in term 6 is even lower than that in term 3 (p-value=0.03).

2.4.3 Organizational History and Individual Behavior

Until now we have mainly focused on aggregated data. To test the hypotheses on the strategic relevance of organizational history in the OL treatments, it is necessary to analyze individual-level data. If a subject in the OL treatments detects uncooperative historical behaviors of the other active member in her current organization, she can punish her opponent base on this historical information. In the NoOL treatments, however, organizational history is not strategically relevant for a newcomer of an organization in the sense that she cannot directly depend on the historical information to punish or reward her opponent. The organizational history in the NoOL treatments can only affect decisions through a learning effect or as a coordination device. In order to disentangle a learning or coordination effect of organizational history from a strategic effect, we compare the effects of historical information on newcomers' decisions in the OL treatments with the effects in the NoOL treatments.

We first look at the comparison between the 1-OL and 1-NoOL treatments. We use mixed effect logistic regressions with three levels: subject, organization, independent matching group. The dependent variable is the action that an *incoming member* subject i takes in round t ($Coop_t^i$). Regressors mainly include a time trend (Round), a one-round lagged dependent variable ($Coop_{t-1}^i$), the action taken by the other member j of subject i's previous organization in round $t-1$ ($Coop_{t-1}^j$), the initial action of the subject i ($Coop_1^i$), and two pieces of historical information for subject i's current organization ($Coop_{t-1}^A$ and $Coop_{t-1}^B$). A denotes the role currently assigned to the incoming member subject i and B is the other role in subject i's current organization. $Coop_{t-1}^A$ stands for the action taken by the member of role A in round $t-1$ and $Coop_{t-1}^B$ stands for the action taken by the member of role B in round $t-1$. Hence, in the 1-OL treatment $Coop_{t-1}^B$ is the action that subject i's current opponent took in round $t-1$, while in the 1-NoOL treatment $Coop_{t-1}^B$ is the action taken in round $t-1$ by the preceding member whom subject i's current opponent replaces. $Coop_{t-1}^A$ always stands for the action taken in round $t-1$ by the preceding member whom subject i replaces.

Table 2.8: Estimates of determinants of cooperative decisions in 1-round treatments

Variables	1-OL $Coop_t^i$	1-NoOL $Coop_t^i$
Round	-0.030***	-0.022***
	(0.007)	(0.008)
$Coop_{t-1}^i$	0.933***	2.168***
	(0.228)	(0.177)
$Coop_{t-1}^j$	0.366*	0.645***
	(0.219)	(0.204)
$Coop_1^i$	1.367***	0.504***
	(0.194)	(0.176)
$Coop_{t-1}^A$	0.097	-0.507*
	(0.247)	(0.259)
$Coop_{t-1}^B$	0.835***	0.009
	(0.209)	(0.223)
Constant	-2.477***	-2.477***
	(0.259)	(0.254)
Indep: sd(_cons)	0.166	0.300
	(0.207)	(0.210)
Org: sd(_cons)	0.595	0.566
	(0.107)	(0.143)
Observations	1,455	2,008

Notes: This table presents the estimates with a mixed-effect logistic model. Random effects are captured by random intercepts grouped by independent matching group and organization. The dependent variable $Coop_t^i$ is the action taken by *an incoming member* subject i in round t. The reported coefficients stand for the marginal effects on the unobserved "latent" dependent variable rather than on $Coop_t^i$. Standard errors are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

The coefficients of $Coop_{t-1}^i$, $Coop_{t-1}^j$, and $Coop_1^i$ are significant and positive in both treatments. A subject is more likely to cooperate if she or the other member in her previous organization cooperated in the previous round. Subjects' initial choice, which can be viewed as a proxy of their cooperative tendency, is also predictive for their later choices.

In the 1-OL treatment, $Coop_{t-1}^B$ has a significantly positive impact. When an incoming member sees that the other member cooperated in the previous round, she is more likely to cooperate, ceteris paribus. This result is consistent with the fact that $Coop_{t-1}^B$ is the strategically most relevant piece of historical information. The significance does not hold for the 1-NoOL treatment. Role B now refers to a member that has already left the organization so that $Coop_{t-1}^B$ is not strategically relevant. To test whether the effect of organizational history is different between the 1-OL and 1-NoOL treatments, we pool the data of the two treatments. The interaction term of the treatment dummy and $Coop^B_{t-1}$ is significant (p-value<0.001). These results support H3.a that incoming members in the 1-OL treatment are more sensitive to organizational history than those in the 1-NoOL treatment. At the same time, the effect in the 1-OL treatment is not large enough to induce significantly high cooperation levels. Calculations reveal that the marginal effect of $Coop^B_{t-1}$ on $Coop^i_t$ is only about 0.09. If a junior member plays C rather than D, this increases the probability that the next junior member plays C by only 9% on average.

In the 3-OL and 3-NoOL treatments, organizational membership changes every three rounds. We use means over last three-round information, $\sum_{k=1}^3 Coop^A_{t-k}/3$ and $\sum_{k=1}^3 Coop^B_{t-k}/3$, as regressors for organizational history. We estimate the case in which the subject i is an incoming member (in term 1).

Table 2.9: Estimates of determinants of cooperative decisions in 3-round treatments

Variables	3-OL $Coop^i_t$	3-NoOL $Coop^i_t$
Round	-0.032**	-0.045***
	(0.015)	(0.014)
$Coop^i_{t-1}$	0.045	1.452***
	(0.482)	(0.414)
$Coop^j_{t-1}$	1.591***	1.638***
	(0.340)	(0.318)
$Coop^i_1$	0.377	1.652***
	(0.340)	(0.318)
$\sum_{k=1}^3 Coop^A_{t-k}/3$	-0.215	-1.085*
	(0.604)	(0.633)
$\sum_{k=1}^3 Coop^B_{t-k}/3$	1.750***	-0.013
	(0.577)	(0.620)
Constant	-1.553***	-1.881***
	(0.455)	(0.684)
Indep: sd(_cons)	0.437	1.532
	(0.380)	(0.504)
Org: sd(_cons)	0.634	1.83e-07
	(0.282)	(0.270)
Observations	321	614

Notes: This table presents the estimates with a mixed-effect logistic model. Random effects are captured by random intercepts grouped by independent matching group and organization. The dependent variable $Coop^i_t$ is the action taken by *an incoming member* subject i in round t. The reported coefficients stand for the marginal effects on the unobserved "latent" dependent variable rather than on $Coop^i_t$. Standard errors are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Similar to the result in the 1-OL and 1-NoOL treatments, $\sum_{k=1}^3 Coop^B_{t-k}/3$ has a significantly positive effect in the 3-OL treatment but not in the 3-NoOL treatment.

We also test that the interaction term of treatment dummy and $\sum_{k=1}^{3} Coop_{t-k}^{B}/3$ is significant (p-value < 0.001). We conclude that incoming members in the 3-OL treatment are more sensitive to organizational history than those in the 3-NoOL treatment.[9]

2.4.4 Robustness Check: the Effect of Re-matching

As outlined in the section of design, when subjects exit an organization they are randomly re-matched to another organization. This means that there is a positive probability that subjects will encounter each other again in a later round. We have matching groups varying in sizes from 6 to 10 subjects and sessions with 10-18 participants, while there are at least 30 rounds of play and 40 rounds of play in expectation. Subjects may realize that they are likely to interact with the same subject again in the future (even though they cannot know when this occurs). If subjects take this into account they may have an incentive to behave cooperatively even in the NoOL treatments. The distinction between the OL and NoOL treatments might thus be somewhat diluted due to the presence of (frequent) re-matching.

To address this issue, we set up additional 3-OL and 3-NoOL treatments with matching groups of 16 or 18 subjects.[10] To further decrease the probability that subjects interacted more than once, we set the minimum number of rounds to 10 (this was 30 in the original treatments) and reduced the continuation probability to 70% (this was 90% in the original treatment).

Moreover, we used rotating matching such that a subject, if possible, was re-matched to a subject she had not played with before. With this new design, the probability that a subject was matched to another subject more than once decreased from close to 100% in the original sessions to less than 5% in these extra sessions. At the same time, cooperative equilibria still exist for organizations with an overlapping membership structure.

The average cooperation rates for these additional treatments are presented in the row "$10 + 70\%$" of Table 2.10. For ease of comparison we also include the cooperation rates for the original treatments, now labeled "$30+90\%$". The results show that the difference in cooperation between the 3-OL and 3-NoOL treatments is more pronounced for the new treatments ("$10 + 70\%$") than for the original treatments ("$30 + 90\%$"). The difference is still not statistically significant though. To rule out the effect of

[9]We have performed additional regressions in which the average historical cooperation rate of an organization was added as an explanatory variable to the models of Tables 2.8 and 2.9. Doing so does not change the result that in the OL treatments an incoming member is more likely to cooperate if the incumbent member cooperated in the last round(s). The effect of the average historical cooperation rates itself is positive, and significantly so (only) in the NoOL treatments.

[10]In total, 10 extra sessions were run in October 2016 and 178 subjects participated. The number of subjects was 18 in nine sessions; one session had 16 subjects. There was one independent matching group per session. Details are reported in Appendix Table 2.13. None of the subjects had participated in any of the earlier sessions.

different numbers of rounds, we also look at average cooperation rates over the first 10 rounds. The difference between the 3-NoOL (10+70%) and 3-OL(10+70%) treatments is not significant either. [11]

Table 2.10: Average cooperation rates by treatment (including additional treatments)

No. rounds	Treatment	3-OL	3-NoOL	p-value
All rounds	30+90%	19.82 (12.40) N = 8	17.37 (15.27) N = 7	0.56
	10+70%	29.33 (16.56) N = 5	17.69 (6.66) N = 5	0.35
First 10 rounds	30+90%	27.69 (14.61) N = 8	25.47 (14.86) N = 7	0.64
	10+70%	32.33 (16.52) N = 5	19.82 (7.68) N = 5	0.21

Notes: Cooperation rates are reported in percentages. The rows of "All rounds" display statistics over all rounds, and the rows of "First 10 rounds" display statistics over the first 10 rounds. An independent matching group is a unit of observation. Standard deviations are in parentheses. N stands for the number of independent matching groups.

Moreover, again cooperation rates display a declining time trend in both the 3-OL and 3-NoOL treatments, and the difference between the two treatments weakens with time (see Figure 2.4 in Appendix). The results about the different cooperation rates for junior and senior terms and for the effect of organizational history on cooperation carry over to the two additional treatments. Results are reported in Tables 2.14 and 2.15 respectively in Appendix.

In summary, the presence of (frequent) re-matching seems to dilute the difference between overlapping and non-overlapping memberships somewhat but does not saliently alter the behavioral patterns related to an overlapping membership structure.

2.5 Conclusion

This paper investigates cooperation in ongoing organizations with overlapping membership structures. Our experimental results provide at best weak support for the pre-

[11] We hypothesized that a more pronounced difference in cooperation would be driven by a decrease in the cooperation rate in the new 3-NoOL (10+70%) treatment compared with the 3-NoOL (30+90%) treatment, because multiple interactions between the same players are less likely in the new treatment. However, the increased (though still insignificant) difference between the 3-OL and 3-NoOL sessions is (partly) driven by an unexpected increase in cooperation in the 3-OL (10+70%) treatment compared with the 3-OL (30+90%). Since this difference is not statistically significant though we do not wish to make too much out of it.

diction that an overlapping membership structure is conducive to cooperation. And this holds irrespective of whether the overlapping memberships are short (1 round) or long (3 rounds). This conclusion is consistent with the results in Offerman *et al.* (2001) who also find relatively low cooperation rates, and with Duffy and Lafky (2016) who find no difference in the contributions between overlapping and "fixed" matching protocols.

Why does an overlapping membership structure fail to induce cooperation in our experiment? One possibility is that our experiment allows for little learning. In our experiment, participants can learn as they move from one organization to the next, but all organizations have only one life. Dal Bó (2005) and Duffy and Ochs (2009) implement indefinitely repeated games with fixed matching and they allow subjects to play multiple of these games. They find that it takes some learning before subjects start to cooperate and before cooperation levels in indefinitely repeated games become significantly higher than those in one-shot games or games with random matching. It cannot be ruled out that cooperation levels will go up if subjects participate in a sequence of overlapping membership games. From an applied perspective, however, one may wonder how realistic such learning possibilities are. It is as if at some point all organizations start all over again.

Another possibility is that cooperation is just harder to sustain with an overlapping matching structure than in comparable repeated games with fixed matching. Some theoretical arguments indeed seem to point in that direction. Bhaskar (1998) points out that cooperation can only be sustained as a subgame perfect equilibrium if players take the complete history of the game into account, which seems rather demanding. Messner and Polborn (2003) indicate that cooperation in overlapping generations games is not robust to small random shocks to the payoffs, unlike cooperation in repeated games with fixed matching. We leave it to future work to perform a direct and integrated comparison of repeated games with fixed matching and repeated games with overlapping matching.

Our study does not include a baseline treatment with fixed matching. To rule out the possibility that it is the payoff structure and continuation probability in our study that lead to a cooperation failure in the OL treatments, we refer to Duffy and Ochs (2009). They use a similar payoff structure to our experiment and the same continuation probability of 90% to implement infinitely repeated prisoner's dilemma games.[12] They show that in the treatment with random matching and no information about the previous action taken by the subject's current opponent, cooperation rates on average start at 43% and approach zero by round 30. These results are consistent with what we observe in the 1-NoOL treatment (see the upper-right graph in Figure

[12]The payoffs they use are (10,10), (0,30), (30,0), (20,20) for actions (D,D), (C,D), (D,C), (C,C) respectively. The size of matching groups in their study is 14 subjects.

2.1). They also display that in the treatment with fixed matching and full history of actions cooperation rates on average start at 48% and rise to above 70% as subjects gain experience. The ascending time trend of cooperation rates supports that our choice of the payoff structure and continuation probability can empirically sustain cooperation in infinitely repeated prisoner's dilemma games with *fixed matching*.

Even though not leading to high levels of cooperation, an overlapping membership structure does generate some notable behavioral patterns in our experiment. Specifically, we find that junior members are significantly more cooperative than senior members. The shadow of the future places at least some constraint on opportunistic behavior. Moreover, we find that junior members are affected by information about past behavior in the organization. Junior members are more likely to cooperate if the senior member they interact with also cooperated as a junior. This indicates that cooperation in an organization is contagious to some extent, and is transmitted from one generation to the next. This may constitute an important component to our understanding of organizational culture.

2.6 Appendix

2.6.A Tables and Figures

Table 2.11: Cooperation rates by independent matching group

Treatment	Session	Indep	Subject	Round	CR	CR (first 30 rounds)
1-OL	3	1	8	35	13.57	15.00
1-OL	3	2	8	35	32.14	33.33
1-OL	4	3	8	56	12.72	20.83
1-OL	4	4	10	56	5.71	6.33
1-OL	5	5	8	47	12.50	17.50
1-OL	5	6	8	47	8.24	10.83
1-OL	6	7	8	41	10.67	11.25
1-OL	6	8	8	41	12.20	15.83
1-NoOL	7	9	6	43	17.44	18.89
1-NoOL	7	10	8	43	17.73	21.25
1-NoOL	8	11	10	31	3.55	3.67
1-NoOL	9	12	8	30	22.92	22.92
1-NoOL	9	13	8	30	10.00	10.00
1-NoOL	10	14	8	42	8.03	6.67
1-NoOL	10	15	8	42	8.63	10.83
3-OL	11	16	10	40	24.50	28.67
3-OL	12	17	8	41	1.52	2.08
3-OL	12	18	8	41	14.63	14.17
3-OL	13	19	6	45	9.63	14.44
3-OL	13	20	8	45	22.78	23.75
3-OL	14	21	6	30	39.44	39.44
3-OL	14	22	6	30	26.67	26.67
3-NoOL	15	23	6	40	7.08	7.78
3-NoOL	15	24	8	40	50.31	50.00
3-NoOL	16	25	8	30	3.33	3.33
3-NoOL	16	26	8	30	17.08	17.08
3-NoOL	17	27	6	39	6.84	8.89
3-NoOL	17	28	6	39	18.80	20.56
3-NoOL	18	29	6	38	25.88	31.67
3-NoOL	18	30	6	38	9.65	10.56

Notes: Column 2 (3) reports the serial numbers of sessions (independent matching groups). Column 4 (5) reports the numbers of subjects (rounds) for all matching groups. Column 6 displays the cooperation rates by independent matching group over all rounds. Column 7 displays the cooperation rates by independent matching group over the first 30 rounds. Cooperation rates are reported in percentages.

Table 2.12: Estimates for the junior-term effect on cooperation

Variables	1-OL $Coop_t^i$	3-OL $Coop_t^i$	1-OL $Coop_t^i$	3-OL $Coop_t^i$
Round	-0.064***	-0.036***	-0.049***	-0.018**
	(0.006)	(0.007)	(0.006)	(0.007)
Junior	0.431***	0.777***	0.500***	1.066***
	(0.133)	(0.169)	(0.140)	(0.186)
$Coop_{t-1}^i$			0.665***	1.537***
			(0.184)	(0.191)
$Coop_{t-1}^j$			0.599***	1.541***
			(0.193)	(0.169)
Constant	-1.620***	-1.777***	-2.174***	-2.624***
	(0.272)	(0.544)	(0.263)	(0.332)
Observations	2976	2046	2910	1,994

Notes: This table presents the estimates with a mixed-effect logistic model. Random effects are captured by random intercepts grouped by organization and independent matching group. The dependent variable is the action subject i takes in round t ($Coop_t^i$). Junior is the dummy variable for whether the subject is currently a junior (first 5 terms) (=1) or a senior (=0). $Coop_{t-1}^i$ the action taken by subject i in round $t-1$. $Coop_{t-1}^j$ is the action taken by subject i's previous opponent j in round $t-1$. Standard errors are in the parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Table 2.13: Cooperation rates by independent matching group in additional treatments

Treatment	Session	Indep	Subject	Round	CR	CR (first 10 rounds)
3-OL (10+70%)	19	31	18	11	14.65	15.56
3-OL (10+70%)	20	32	18	11	48.99	51.67
3-OL (10+70%)	23	33	18	18	13.58	21.11
3-OL (10+70%)	25	34	18	10	25.00	25.00
3-OL (10+70%)	26	35	18	12	44.44	48.33
3-NoOL (10+70%)	21	36	18	13	8.97	10.56
3-NoOL (10+70%)	22	37	18	11	18.18	20.00
3-NoOL (10+70%)	24	38	16	11	21.02	21.88
3-NoOL (10+70%)	27	39	18	12	26.39	31.11
3-NoOL (10+70%)	28	40	18	12	13.89	15.56

Notes: Column 2 (3) reports the serial numbers of sessions (independent matching groups). Column 4 (5) reports the numbers of subjects (rounds) for all matching groups. Column 6 displays the cooperation rates by independent matching group in additional treatments over all rounds. Column 7 displays the cooperation rates by independent matching group in additional treatments over the first 10 rounds. Cooperation rates are reported in percentages.

Table 2.14: Cooperation rates by junior and senior terms in additional treatments

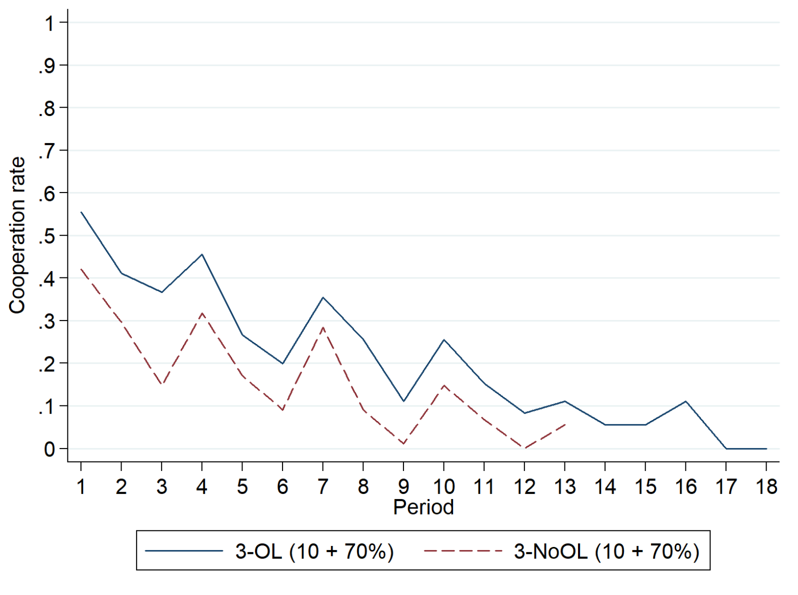
Treatment	Junior	Senior	p-value
3-OL (10+70%)	31.52	11.63	0.04

Notes: Cooperation rates are reported in percentages. Column 4 displays the p-values of the two-sided signed-rank tests comparing subjects' senior and junior terms.

Table 2.15: Estimates of determinants of cooperative decisions in additional treatments

Variables	3-OL (10 + 70%) $Coop_t^i$	3-NoOL (10 + 70%) $Coop_t^i$
Round	-0.056	-0.192***
p	(0.070)	(0.074)
$Coop_{t-1}^i$	0.640	-0.297
	(0.519)	(0.604)
$Coop_{t-1}^j$	0.208	0.237
	(0.511)	(0.575)
$Coop_1^i$	1.270***	1.702***
	(0.415)	(0.369)
$\sum_{k=1}^3 Coop_{t-k}^A/3$	-1.083	0.698
	(0.763)	(0.709)
$\sum_{k=1}^3 Coop_{t-k}^B/3$	1.571**	-0.424
	(0.755)	(0.74)
Constant	-1.200	-0.897
	(0.822)	(0.664)
Observations	153	282

Notes: This table presents the estimates with the same model as in Table 2.9 for the two additional treatments (10+70%). Standard errors are in the parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.



This figure shows how cooperation rates evolve over time in the additional treatments. The solid line is for 3-OL (10 + 70%) and the dashed line is for 3-NoOL (10 + 70%).

Figure 2.4: Cooperation rates over time in additional treatments

2.6.B Instructions

(*1-OL Treatment*)

Instructions

Welcome to our experiment. Please follow the instructions carefully. During the experiment, your earnings are denoted in points. Your final earnings in the experiment consist of two parts: a show-up fee EUR 2.5 and the points you earn by making decisions. The latter part is determined by your own decisions, the decisions of other participants and chance. Every point is worth EUR 3 in the experiment. You will receive your final earnings by transfer within three working days after the experiment.

Decisions and Earnings

The experiment consists of several rounds. In each round, you play a game with one other participant. You have to choose between A and B, and so does the participant you play with. If you choose A and so does the other participant, both of you will earn 2 points; if you choose A and the other participant chooses B, you will earn 0 and the other participant will earn 3 points; if you choose B and the other participant chooses A, you will earn 3 points and the other participant will earn 0; if both of you choose B, both of you will earn 1 point.

Your decision	The other's decision	Your earnings	The other's earnings
A	A	2	2
A	B	0	3
B	A	3	0
B	B	1	1

Organizations and Matching

In the experiment there are multiple organizations. In each round, each organization has two members. The two members play the game described above, and make earnings in accordance with the choices they make. The membership of the organization changes from round to round. To be precise the following rules apply

- you are a member of one organization for exactly two consecutive rounds
- after the two rounds you switch to another organization
- after you leave an organization, the new organization you will switch to is randomly selected (after several switches, it is possible that you move back to an organization where you have played already before)
- the other member you play with in the first round of an organization is different from the one you play with in the second round: the member you play with in the first round will leave the organization after that round and be replaced with a new member; in turn after you play with that new member, you will leave the organization

- when you switch to a new organization, the member you will play with has already played in that organization for one round

The membership structure of a specific organization can be depicted by means of the following figure. [1] Same colors denote same participants.

Round	1	2	3	4	5	6	7	8	…
Member 1									…
Member 2									…

For example, the "dark green" participant stays in this organization in Round 5 and 6. In Round 5, he plays with the "light green" participant who is already a member of this organization in Round 4. When Round 5 ends, the "light green" participant switches to another organization and is replaced by the "light grey" participant. Hence in Round 6, the "dark green" participant plays with the "light grey" participant. Then the "dark green" participant switches to another organization and is replaced by the "purple" participant. The change of organizational membership goes on like this.

Rounds and Earnings

The experiment consists of at least 30 rounds in total. After the 30^{th} round, how many rounds the experiment will last is determined randomly. Starting from the 30^{th} round, each time one round has been completed, the computer will randomly draw a number between 1 and 100. If the number is below or equal to 90, the experiment will continue for one more round. Hence, the probability that the experiment continues for at least one more round after the 30^{th} round is 90%.

You play for quite a few rounds, but only two rounds will be paid: two rounds are randomly chosen and your earnings in the two rounds are added up.

Information

At the beginning of each round, you will see a message to remind you in that round whether you are in another organization.

After that, the decision history of your current organization will be displayed on the screen. **The decision history of an organization consists of decisions made by preceding members in that organization for previous rounds.**

[1] The ONLY special case is that ONE member in each organization has to switch after the 1^{st} round of the experiment, which doesn't satisfy the rule of switching every two rounds.

Take the organization depicted by the colorful figure as an example. In Round 5, the "dark green" participant comes to this organization to **replace the "yellow" participant** and **plays with the "light green" participant**. Before making decisions, **both of the "dark green" and "light green" participants can see** the organizational history consisting of decisions: made by **the "yellow" and the "light green" participants in Round 4**, made by the "black" and the "yellow" participants in Round 3; made by the "red" and the "black" participants in Round 2; made by the "blue" and the "red" participants in Round 1.

Hence it means, when you switch to a new organization, the last record of the history consists of two last-round decisions. **One is made by the member who is replaced by you**; **the other is made by the member who you will play with**. Besides, when you stay at the same organization as in the previous round, the last record of the history includes **your own last-round decision**.

At the end of each round, you will be informed about your earnings of this round and the other member's earnings of this round.

Summary

- The points you earn in the experiment are determined by your own decisions, the decisions of other participants and the chance which determines the rounds that will be paid.

-There are multiple organizations in the experiment.

- Every two rounds you are randomly reassigned to another organization.

- In the two consecutive rounds when you are in the same organization, you play with different members.

- You will see the decision history of your current organization at the beginning of each round.

- How many more rounds than 30 the experiment will last is randomly determined.

Procedure and Questions

After reading the instructions, you will be asked to answer a few control questions to check your understanding. The first round of the experiment will start as soon as all the participants have answered the control questions. In the end of the experiment, please wait until your computer ID number is called and fill in payment details.

Please be quiet during the entire experiment and do not talk to your neighbors. If you have a question, please raise your hand and you will be answered privately.

(*1-NoOL Treatment*)

Instructions

Welcome to our experiment. Please follow the instructions carefully. During the experiment, your earnings are denoted in points. Your final earnings in the experiment consist of two parts: a show-up fee EUR 2.5 and the points you earn by making decisions. The latter part is determined by your own decisions, the decisions of other participants and chance. Every point is worth EUR 3 in the experiment. You will receive your final earnings by transfer within three working days after the experiment.

Decisions and Earnings

The experiment consists of several rounds. In each round, you play a game with one other participant. You have to choose between A and B, and so does the participant you play with. If you choose A and so does the other participant, both of you will earn 2 points; if you choose A and the other participant chooses B, you will earn 0 and the other participant will earn 3 points; if you choose B and the other participant chooses A, you will earn 3 points and the other participant will earn 0; if both of you choose B, both of you will earn 1 point.

Your decision	The other's decision	Your earnings	The other's earnings
A	A	2	2
A	B	0	3
B	A	3	0
B	B	1	1

Organizations and Matching

In the experiment there are multiple organizations. In each round, each organization has two members. The two members play the game described above, and make earnings in accordance with the choices they make. The membership of the organization changes from round to round. To be precise the following rules apply

- you are a member of one organization for exactly one round
- after each round you switch to another organization
- after you leave an organization, the new organization you will switch to is randomly selected (after several switches, it is possible that you move back to an organization where you have played in already before)

The membership structure of a specific organization can be depicted by means of the following figure. Same colors denote same participants.

Round	1	2	3	4	5	6	7	8	…
Member 1									
Member 2									…

For example, the "black" participant stays in this organization in Round 5. In Round 5, he plays with the "white" participant who is also a newcomer of this organization. When Round 5 ends, they both switch to other organizations and are replaced by the "light grey" and the "olive" participants. The change of organizational membership goes on like this.

Rounds and Earnings

The experiment consists of at least 30 rounds in total. After the 30th round, how many rounds the experiment will last is determined randomly. Starting from the 30th round, each time one round has been completed, the computer will randomly draw a number between 1 and 100. If the number is below or equal to 90, the experiment will continue for one more round. Hence, the probability that the experiment continues for at least one more round after the 30th round is 90%.

You play for quite a few rounds, but only two rounds will be paid: two rounds are randomly chosen and your earnings in the two rounds are added up.

Information

At the beginning of each round, the decision history of your current organization will be displayed on the screen. **The decision history of an organization consists of decisions made by preceding members in that organization for previous rounds.**

At the end of each round, you will be informed about your earnings of this round and the other member's earnings of this round.

Summary

- The points you earn in the experiment are determined by your own decisions, the decisions of other participants and chance which determines the rounds that will be paid.

-There are multiple organizations in the experiment.

- Every round you are randomly reassigned to another organization.

- In two consecutive rounds, you play with different members.

- You will see the decision history of your current organization at the beginning of each round.
- How many more rounds than 30 the experiment will last is randomly determined.

Procedure and Questions

After reading the instructions, you will be asked to answer a few control questions to check your understanding. The first round of the experiment will start as soon as all the participants have answered the control questions. In the end of the experiment, please wait until your computer ID number is called and fill in payment details.

Please be quiet during the entire experiment and do not talk to your neighbors. If you have a question, please raise your hand and you will be answered privately.

(*3-OL Treatment*)

Instructions

Welcome to our experiment. Please follow the instructions carefully. During the experiment, your earnings are denoted in points. Your final earnings in the experiment consist of two parts: a show-up fee EUR 2.5 and the points you earn by making decisions. The latter part is determined by your own decisions, the decisions of other participants and chance. Every point is worth EUR 3 in the experiment. You will receive your final earnings by transfer within three working days after the experiment.

Decisions and Earnings

The experiment consists of several rounds. In each round, you play a game with one other participant. You have to choose between A and B, and so does the participant you play with. If you choose A and so does the other participant, both of you will earn 2 points; if you choose A and the other participant chooses B, you will earn 0 and the other participant will earn 3 points; if you choose B and the other participant chooses A, you will earn 3 points and the other participant will earn 0; if both of you choose B, both of you will earn 1 point.

Your decision	The other's decision	Your earnings	The other's earnings
A	A	2	2
A	B	0	3
B	A	3	0
B	B	1	1

Organizations and Matching

In the experiment there are multiple organizations. In every round, each organization has two members. The two members play the game described above, and make earnings in accordance with the choices they make. The membership of the organization changes every three rounds. To be precise the following rules apply

- you are a member of one organization for exactly six consecutive rounds
- after the six rounds you switch to another organization
- after you leave an organization, the new organization you will switch to is randomly selected (after several switches, it is possible that you move back to an organization where you have played already before)
- the other member you play with in the first three rounds of an organization is different from the one you play with in the last three rounds: the member you play with in the first three rounds will leave the organization after the third round and be replaced with a new member; in turn after you play with that new member for three rounds, you will leave the organization
- when you switch to a new organization, the member you will play with has already played in that organization for three rounds

The membership structure of a specific organization can be depicted by means of the following figure.[2] Same colors denote same participants.

Round	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	…
Member 1																						…
Member 2																						…

For example, the "dark green" participant stays in this organization in Round 13-18. In Round 13-15, he plays with the "light green" participant who is already a member of this organization in Round 10-12. When Round 15 ends, the "light green" participant switches to another organization and is replaced by the "light grey" participant. Hence in Round 16-18, the "dark green" participant plays with the "light grey" participant. Then the "dark green" participant switches to another organization and is replaced by the "purple" participant. The change of organizational membership goes on like this.

Rounds and Earnings

[2] The ONLY special case is that one member in each organization has to switch after the 3rd round of the experiment, which doesn't satisfy the rule of switching every six rounds.

The experiment consists of at least 30 rounds in total. After the 30th round, how many rounds the experiment will last is determined randomly. Starting from the 30th round, each time one round has been completed, the computer will randomly draw a number between 1 and 100. If the number is below or equal to 90, the experiment will continue for one more round. Hence, the probability that the experiment continues for at least one more round after the 30th round is 90%.

You will play for quite a few rounds, but only two rounds are randomly chosen for your earnings in the experiment: randomly choose two rounds and add up your earnings in the two rounds.

Information

For each round, you will see a message to remind you in that round whether you are in another organization and whether you play with a new member.

Besides, the decision history of your current organization will be displayed on the screen. **The decision history of an organization consists of decisions made by preceding members in that organization for previous rounds.**

Take the organization depicted by the colorful figure as an example. In Round 13, the "dark green" participant comes to this organization to **replace the "yellow" participant** and **plays with the "light green" participant**. Before making decisions, **both of the "dark green" and "light green" participants can see** the organizational history consisting of decisions: made by **the "yellow" and the "light green" participants in Round 10-12**, made by the "black" and the "yellow" participants in Round 7-9; made by the "red" and the "black" participants in Round 4-6; made by the "blue" and the "red" participants in Round 1-3.

Hence it means, when you switch to a new organization, the last three records of the history consists of six past decisions. **Three past decisions displayed are made by the member who is replaced by you**; **the other three are made by the member who you will play with**. In addition, when you stay at the same organization as in the previous round, the organizational history **certainly includes your own past decisions** and **may contains past decisions of the member you will play with.**

At the end of each round, you will be informed about your earnings of this round and the other member's earnings of this round.

Summary

- The points you earn in the experiment are determined by your own decisions, the decisions of other participants and the chance which determines the rounds that will be paid.

- There are multiple organizations in the experiment.

- Every six rounds you are randomly reassigned to another organization.

- In the six consecutive rounds when you are in the same organization, you play with two different members.
- You will see the decision history of your current organization at the beginning of each round.
- How many more rounds than 30 the experiment will last is randomly determined.

Procedure and Questions

After reading the instructions, you will be asked to answer a few control questions to check your understanding. The first round of the experiment will start as soon as all the participants have answered the control questions. In the end of the experiment, please wait until your computer ID number is called and fill in payment details.

Please be quiet during the entire experiment and do not talk to your neighbors. If you have a question, please raise your hand and you will be answered privately.

(*3-NoOL Treatment*)

Instructions

Welcome to our experiment. Please follow the instructions carefully. During the experiment, your earnings are denoted in points. Your final earnings in the experiment consist of two parts: a show-up fee EUR 2.5 and the points you earn by making decisions. The latter part is determined by your own decisions, the decisions of other participants and chance. Every point is worth EUR 3 in the experiment. You will receive your final earnings by transfer within three working days after the experiment.

Decisions and Earnings

The experiment consists of several rounds. In each round, you play a game with one other participant. You have to choose between A and B, and so does the participant you play with. If you choose A and so does the other participant, both of you will earn 2 points; if you choose A and the other participant chooses B, you will earn 0 and the other participant will earn 3 points; if you choose B and the other participant chooses A, you will earn 3 points and the other participant will earn 0; if both of you choose B, both of you will earn 1 point.

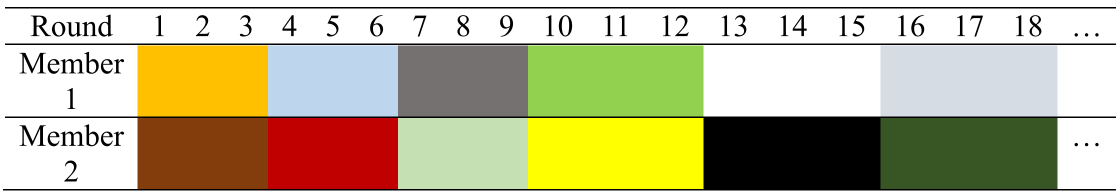
Your decision	The other's decision	Your earnings	The other's earnings
A	A	2	2
A	B	0	3
B	A	3	0
B	B	1	1

Organizations and Matching

In the experiment there are multiple organizations. In each round, each organization has two members. The two members play the game described above, and make earnings in accordance with the choices they make. The membership of the organization changes every three rounds. To be precise the following rules apply

- you are a member of one organization for exactly three consecutive rounds
- after the three rounds you switch to another organization
- after you leave an organization, the new organization you will switch to is randomly selected (after several switches, it is possible that you move back to an organization where you have played in already before)

The membership structure of a specific organization can be depicted by means of the following figure. Same colors denote same participants.



For example, the "black" participant stays in this organization in Round 13-15. In Round 13-15, he plays with the "white" participant who also stays in this organization during these rounds. When Round 15 ends, they switch to other organizations[3] and are replaced by the "light grey" and the "olive" participants. The change of organizational membership goes on like this.

[3] Pay attention that the "white" and "black" participants switch to two different organizations.

Rounds and Earnings

The experiment consists of at least 30 rounds in total. After the 30th round, how many rounds the experiment will last is determined randomly. Starting from the 30th round, each time one round has been completed, the computer will randomly draw a number between 1 and 100. If the number is below or equal to 90, the experiment will continue for one more round. Hence, the probability that the experiment continues for at least one more round after the 30th round is 90%.

You will play for quite a few rounds, but only two rounds are randomly chosen for your earnings in the experiment: randomly choose two rounds and add up your earnings in the two rounds.

Information

For each round, you will see a message to remind you in that round whether you are in another organization.

Besides, the decision history of your current organization will be displayed on the screen. **The decision history of an organization consists of decisions made by preceding members in that organization for previous rounds.**

At the end of each round, you will be informed about your earnings of this round and the other member's earnings of this round.

Summary

- The points you earn in the experiment are determined by your own decisions, the decisions of other participants and chance which determines the rounds that will be paid.

- There are multiple organizations in the experiment.

- Every three rounds you are randomly reassigned to another organization and then play with a different participant.

- You will see the decision history of your current organization at the beginning of each round.

- How many more rounds than 30 the experiment will last is randomly determined.

Procedure and Questions

After reading the instructions, you will be asked to answer a few control questions to check your understanding. The first round of the experiment will start as soon as all the participants have answered the control questions. In the end of the experiment, please wait until your computer ID number is called and fill in payment details.

Please be quiet during the entire experiment and do not talk to your neighbors. If you have a question, please raise your hand and you will be answered privately.

Instructions (3-OL additional sessions)

Welcome to our experiment. Please follow the instructions carefully. During the experiment, your earnings are denoted in points. Your final earnings in the experiment consist of two parts: a show-up fee of EUR 2.5 and the points you earn by making decisions. The latter part is determined by your own decisions, the decisions of other participants and chance. Every point is worth EUR 3 in the experiment. You will receive your final earnings by bank transfer within three working days after the experiment.

Decisions and Earnings

The experiment consists of several rounds. In each round, you play a game with another participant. You choose between A and B, and so does the participant you play with. If you choose A and so does the other participant, both of you will earn 2 points; if you choose A and the other participant chooses B, you will earn 0 and the other participant will earn 3 points; if you choose B and the other participant chooses A, you will earn 3 points and the other participant will earn 0; if both of you choose B, both of you will earn 1 point.

Your decision	The other's decision	Your earnings	The other's earnings
A	A	2	2
A	B	0	3
B	A	3	0
B	B	1	1

Organizations and Matching

In the experiment there are multiple organizations. In each round, each organization has two members. The two members play the game described above, and make earnings in accordance with the choices they make. The membership of the organization changes every three rounds. To be precise the following rules apply

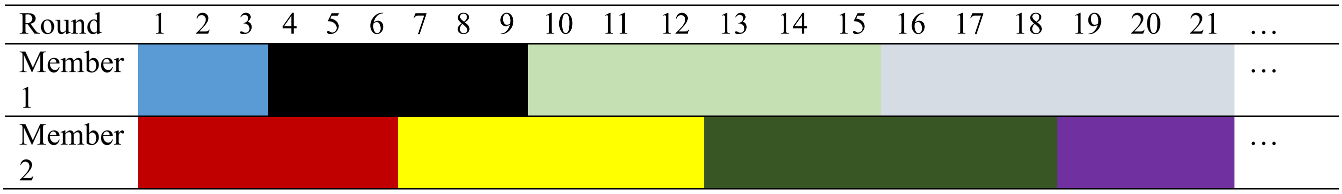
- you are a member of one organization for exactly six consecutive rounds
- after the six rounds you switch to another organization[1]
- the new organization you will switch to is randomly selected (after several switches, it is possible that you move back to an organization in which you have played before)
- the participant you play with in your first three rounds in an organization is different from the one you play with in your last three rounds in the organization: the participant you play with in the first three rounds will leave the organization after the third round and be replaced with another participant (a new member); in turn after you play with

[1] The ONLY exception is that one member in each organization has to switch after the 3rd round of the experiment (see the figure next page), which doesn't satisfy the rule of switching every six rounds.

the new member for three rounds, you will leave the organization

- when you switch to a new organization, the participant you will play with has played in the organization for three rounds

The membership structure of an organization can be depicted by the following figure. Same colors denote same participants.



For example, the "dark green" participant plays in this organization in round 13-18. In round 13-15, he or she plays with the "light green" participant who is already in this organization in round 10-12. When the 15th round ends, the "light green" participant switches to another organization and he or she is replaced by the "light grey" participant. So in round 16-18, the "dark green" participant plays with the "light grey" participant. The change of organizational membership goes on like this.

Rounds and Earnings

The experiment consists of at least 10 rounds in total. After the 10th round, how many rounds the experiment will last is determined randomly. Starting from the 10th round, each time one round has been completed, the computer will randomly draw a number between 1 and 100. If the number is below or equal to 70, the experiment will continue for one more round. So the probability that the experiment continues for at least one more round after the 10th round is 70%.

After you finish the play with a participant for three rounds, if possible, you will be matched with a participant you have not played with before. So the chance you will play with a participant you played with before is less than 5%.

You will play for quite a few rounds, but only two rounds are randomly chosen for your earnings in the experiment. The sum of your earnings in the two rounds chosen is what you earn by making decisions.

Information

For each round, you can see a message for whether you are in another organization.

Besides, the decision history of your current organization is displayed. **The decision history of an organization consists of decisions made by preceding members of the organization in previous rounds.**

Take the figure in the previous page as an example. In the 13th round, both the **"dark green" and "light green" participants can see** the organizational history consisting of decisions: made by the "yellow" and **"light green" participants in round 10-12**, made by the "black" and "yellow" participants in round 7-9; made by the "red" and "black" participants in round 4-6; made by the "blue" and the "red" participants in round 1-3.

So when you switch to a new organization, the organizational history for the last three rounds consists of **three decisions of the participant who is replaced by you** and **three decisions of the participant you will play with**. When you play in the same organization as in the last round, the organizational history **certainly includes your last-round decision**.

At the end of each round, you will be informed about your earnings and the other member's earnings in this round.

Summary

- The points you earn in the experiment are determined by your own decisions, the decisions of other participants and the chance which determines the rounds that will be paid.

- There are multiple organizations in the experiment.

- Every six rounds you are randomly reassigned to another organization.

- In the six consecutive rounds when you are in the same organization, you play with two different participants.

- You can see the decision history of your current organization at the beginning of each round.

- How many more rounds than 10 the experiment will last is randomly determined.

Procedure and Questions

After reading the instructions, you will be asked to answer a few control questions to check your understanding. The first round of the experiment will start as soon as all the participants have answered the control questions. At the end of the experiment, please wait until your seat number is called and fill in payment details.

Please be quiet during the entire experiment and do not talk to your neighbors. If you have a question, please raise your hand and you will be answered privately.

Instructions (3-NoOL additional sessions)

Welcome to our experiment. Please follow the instructions carefully. During the experiment, your earnings are denoted in points. Your final earnings in the experiment consist of two parts: a show-up fee of EUR 2.5 and the points you earn by making decisions. The latter part is determined by your own decisions, the decisions of other participants and chance. Every point is worth EUR 3 in the experiment. You will receive your final earnings by bank transfer within three working days after the experiment.

Decisions and Earnings

The experiment consists of several rounds. In each round, you play a game with another participant. You choose between A and B, and so does the participant you play with. If you choose A and so does the other participant, both of you will earn 2 points; if you choose A and the other participant chooses B, you will earn 0 and the other participant will earn 3 points; if you choose B and the other participant chooses A, you will earn 3 points and the other participant will earn 0; if both of you choose B, both of you will earn 1 point.

Your decision	The other's decision	Your earnings	The other's earnings
A	A	2	2
A	B	0	3
B	A	3	0
B	B	1	1

Organizations and Matching

In the experiment there are multiple organizations. In each round, each organization has two members. The two members play the game described above, and make earnings in accordance with the choices they make. The membership of the organization changes every three rounds. To be precise the following rules apply

- you are a member of one organization for exactly three consecutive rounds
- after the three rounds you switch to another organization
- the new organization you will switch to is randomly selected (after several switches, it is possible that you move back to an organization in which you have played before)

The membership structure of an organization can be depicted by the following figure. Same colors denote same participants.

Round	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	…
Member 1																			…
Member 2																			…

For example, the "black" and "white" participant play in this organization in round 13-15. When the 15th round ends, they switch to other organizations[1] and are replaced by the "light grey" and "dark green" participants. The change of organizational membership goes on like this.

Rounds and Earnings

The experiment consists of at least 10 rounds in total. After the 10th round, how many rounds the experiment will last is determined randomly. Starting from the 10th round, each time one round has been completed, the computer will randomly draw a number between 1 and 100. If the number is below or equal to 70, the experiment will continue for one more round. So the probability that the experiment continues for at least one more round after the 10th round is 70%.

After you finish the play with a participant for three rounds, if possible, you will be matched with a participant you have not played with before. So the chance you will play with a participant you played with before is less than 5%.

You will play for quite a few rounds, but only two rounds are randomly chosen for your earnings in the experiment. The sum of your earnings in the two rounds chosen is what you earn by making decisions.

Information

For each round, you can see a message for whether you are in another organization.

Besides, the decision history of your current organization is displayed. **The decision history of an organization consists of decisions made by preceding members of the organization in previous rounds.**

At the end of each round, you will be informed about your earnings and the other member's earnings in this round.

[1] Pay attention that the "white" and "black" participants switch to two different organizations.

Summary

- The points you earn in the experiment are determined by your own decisions, the decisions of other participants and chance which determines the rounds that will be paid.

- There are multiple organizations in the experiment.

- Every three rounds you are randomly reassigned to another organization and play with a different participant.

- You can see the decision history of your current organization at the beginning of each round.

- How many more rounds than 10 the experiment will last is randomly determined.

Procedure and Questions

After reading the instructions, you will be asked to answer a few control questions to check your understanding. The first round of the experiment will start as soon as all the participants have answered the control questions. At the end of the experiment, please wait until your seat number is called and fill in payment details.

Please be quiet during the entire experiment and do not talk to your neighbors. If you have a question, please raise your hand and you will be answered privately.

Chapter 3

The Effect of Decentralized Punishment on Centralized Sanctioning Institutions: An Experimental Study

3.1 Introduction

In social dilemma situations, the pursuit of individual interests conflicts with the maximization of social welfare, yielding free-riding problems and low cooperation. One way of governing free riders is to formally transfer sanctioning power to a centralized authority. For example, villagers turn to their heads for adjudicating disputes over local resources exploitations. Leaders of work teams are appointed to discipline workers who shirk. Managers are given the power to determine the share of bonus among subordinates. In view of their importance in practice, centralized sanctioning institutions in which a legitimate enforcer is responsible for deterring free riders are widely explored (Alchian and Demsetz 1972; Vyrastekova and Van Soest 2003; Fehr and Fischbacher 2004; Güth *et al.* 2007; Anielski *et al.* 2009; Van der Heijden *et al.* 2009; O'Gorman *et al.* 2009; Baldassarri and Grossman 2011; Cox *et al.* 2013; Stoddard *et al.* 2014). Most of these centralized institutions prove to be effective in alleviating free-riding problems and improving cooperation.

A common feature of these studies is that they rule out the presence of decentralized punishment. They do not take into account the possibility that people can informally castigate all others, including the enforcer.[1] The possibility of hurting the enforcer might affect the enforcer's sanctioning decisions and group cooperation. Thus this paper addresses two research questions. 1) How will the enforcer's centralized sanctions be affected by the possibility of getting punished? 2) How will cooperation under the enforcer's regulations change with the possibility of decentralized punishment?

It is not unusual to see that enforcers get counter-punished by those who are sanctioned by them.[2] An example is the workplace retaliation on bosses, managers

[1]In some of the studies, in which group memberships are fixed, subjects can punish all other group members by contributing less, but the enforcer cannot be targeted for punishment based on her prior sanctions.

[2]The enforcer is also likely to be punished if she sanctions cooperators or fails to sanction free

or team leaders. If an employee is publicly criticized or his bonus is cut by his boss, he might want to let his boss suffer as well. There are various informal ways to counter-punish the boss, e.g. spreading rumors, sending anonymous SMS, scratching the car of the boss or even physically hurting the boss. A number of empirical and theoretical researches in management do find that if employees feel that they are mistreated by their bosses, they are more likely to undermine their bosses in private or publicly challenge them and they feel justified to do so (Folger 1993; Bies and Tripp 1998; Mitchell and Ambrose 2007). Another example is the revenge on policemen. Policemen who collect fines from lawbreakers or arrest criminals may well face the retaliation by these lawbreakers and criminals.

With realistic threats of retaliation as described above, the enforcer might be less willing to sanction other members to avoid potential hurt. But is this conducive or detrimental to cooperation? The answer can be dependent on whether the enforcer is corruptible. Corruption includes various dishonest or illegal behaviors by people who have authority, such as bribery, embezzlement, fraud, and extortion (Klitgaard 1988). In my study, I focus on embezzlement which means theft or appropriation of public resources by people with authority (Amundsen 1999). If the enforcer has no chance to embezzle, the presence of decentralized punishment can erode cooperation. As is found in Nikiforakis (2008), the possibility of counter-punishment makes the enforcer less willing to implement centralized sanctions on free riders, leading to lower cooperation. If the enforcer has chance to embezzle or simply say that she is corruptible, the presence of decentralized punishment is likely to help cooperation. Previous studies point out that, if the enforcer can determine the allocation of generated surplus, she has an incentive to keep a larger portion for herself (Van der Heijden *et al.* 2009; Stoddard *et al.* 2014; Van Leeuwen *et al.* 2015). Such corruptive behaviors as embezzlement can be detrimental to cooperation, because members might lower contributions to reduce potential unfairness in earnings. If other members can monitor and punish the enforcer, she is likely to decrease corruptive behaviors and excessive (anti-social) sanctions, which might be conducive to cooperation.

In answering the research questions, I look at both non-corruptible and corruptible enforcers. I conduct an experiment with a 2×2 design, varying whether the enforcer is corruptible and whether the decentralized punishment possibility is present. Basically, subjects make decisions in a social dilemma situation. When the enforcer is corruptible, the enforcer has a chance to embezzle money by reducing others' benefits. When the possibility of decentralized punishment is present, subjects can reduce other members' earnings with certain cost.

The results show different effects of the possibility of decentralized punishment for

riders, because people might want to implement a social norm of fairness and justice. But the social norm enforcement effect of second-order punishment is not strongly supported by evidences in previous studies (e.g. Cinyabuguma *et al.* 2006; Denant-Boemont *et al.* 2007).

non-corruptible and corruptible enforcers. On the one hand, a non-corruptible enforcer implements weaker sanctions with the possibility of decentralized punishment. But cooperation does not decrease saliently, since the emergence of decentralized punishment on free riders offsets the decrease in centralized sanctions on free riders. On the other hand, in the case with a corruptible enforcer, cooperation decreases significantly with the possibility of decentralized punishment. The main reason is that even though the size of centralized sanctions is not affected, the pro-sociality of centralized sanctions is weakened by the possibility of decentralized punishment.

My paper relates to three strands of literature. First, it contributes to the literature on centralized enforcers by allowing other people to respond to the enforcer's sanctions. My results indicate that leaving out the possibility of decentralized punishment from the analysis can lead to an overestimation of the centralized institution's effectiveness on cooperation. Second, it expands the scope for exploring the effect of second-order punishment on cooperation (Cinyabuguma *et al.* 2006; Denant-Boemont *et al.* 2007; Nikiforakis 2008; Balafoutas *et al.* 2014). Previous studies mainly focus on whether the effectiveness of decentralized punishment is affected by second-order punishment.[3] I study whether the effectiveness of centralized sanctions is affected by second-order punishment. Moreover, I look at this effect for both non-corruptible and corruptible centralized enforcers. Third, it contributes to the literature on the interaction between centralized (formal) and decentralized (informal) sanctions (e.g. Kube and Traxler 2011; Andreoni and Gee 2012). The formal institutions they study are simple regulating rules of punishment on noncompliance, which is different from the case in which a real person implements sanctions. For a simple regulating rule, not only is there no uncertainty of centralized sanctions on noncompliance once detected, but also there is no room for corruptibility.

To the best of my knowledge, only one paper Van Leeuwen *et al.* (2015) has a focus close to my study in the sense that they also allow other players to respond to the leader's use or abuse of leadership power. But in their study, the form of other players' responses is ostracism by majority voting instead of decentralized punishment.

3.2 Experimental Design, Hypotheses and Procedure

3.2.1 Design

In the experiment, I use a variation of a linear public goods game with 4 group members for social dilemma situations. After group members make decisions on investing

[3] Balafoutas *et al.* (2014) investigate whether the effectiveness of third-party punishment on cooperation is affected by counter-punishment.

in public goods, the enforcer, who is randomly chosen, implements sanctions by redistributing members' points earned in the public goods game. This form of sanctioning is related to allowing a leader to determine each member's share of group output (Van der Heijden *et al.* 2009; Cox *et al.* 2013; Van Leeuwen *et al.* 2015). A non-corruptible enforcer can only redistribute among other members' earnings so that she cannot transfer others' money to herself, while a corruptible enforcer can redistribute all members' earnings, which generates room for her to embezzle money. After the enforcer implements sanctions, in some treatments there is possibility for members to punish each other. In particular, members can punish each other by inducing a constant cost to punishment ratio of 1/3.[4]

There are four treatments in the experiment, NoCorrupt/NoDP, NoCorrupt/DP, Corrupt/NoDP, and Corrupt/DP. They are displayed in Table 3.1. In all treatments, subjects play a multiple-stage game, which varies with the treatment.

Table 3.1: Summary of treatments

	No corruptibility	Corruptibility
No decentralized punishment	NoCorrupt/NoDP	Corrupt/NoDP
Decentralized punishment	NoCorrupt/DP	Corrupt/DP

In the first stage of all treatments, each of Members 1, 2, and 3 receives 30 points and Member 4 receives 20 points. In other words, once the enforcer is selected, there assumes to be a lump-sum cost, here equal to 10 points, for Member 4 to enforce centralized sanctions.[5] Members 1, 2, and 3 each decide how much of their endowment they want to contribute to the group account. Member 4 does not make a decision in this stage, and the purpose is to make Member 4 a truly fair-minded and "benevolent" enforcer in treatments with no corruptibility by ruling out the concern that she free rides in the public goods game. The return from the group account depends on the total amount of points that Members 1, 2, and 3 contribute. Specifically, every point a member contributes to the group account gives each group member a return of 0.55 points, that is, the marginal per capita return (MPCR) is 0.55. Denote the contribution of Member i as c_i. The earnings of Member i in the first stage are as follows.

$$\pi_i^1 = \begin{cases} 30 - c_i + 0.55 \times \sum_{k=1}^{3} c_k & \text{if } i = 1,2,3 \\ 20 + 0.55 \times \sum_{k=1}^{3} c_k & \text{if } i = 4 \end{cases}$$

[4]The constant cost to punishment ratio of 1/3 has been applied to several other experiments (e.g. Fehr and Gächter 2002; Gächter *et al.* 2008; Herrmann *et al.* 2008).

[5]As you will see in appendix B, the level of Member 4's endowment, 20 points, can sustain a cooperative subgame perfect equilibrium *easier* in the NoCorrupt/NoDP (Corrupt/DP) treatment than in the NoCorrupt/DP (Corrupt/NoDP) treatment. And the motivation is to make theoretical predictions consistent with the previous intuitive argument, that is, the possibility of decentralized punishment erodes cooperation when the enforcer cannot embezzle while improves cooperation when the enforcer is corruptible.

Corruptibility

In the second stage, Members 1, 2, and 3 do not make a decision in this stage. Member 4 first chooses how many points to collect from members, and then decides how to allocate the collected points to them. Points collected from Member i range between zero and $(\pi_i^1 - 10)$. In other words, after this stage, each member at least keeps 10 points in his private account.

For treatments with *no corruptibility*, Member 4 collects and allocates points among *Members 1, 2, and 3*. Denote the number of points collected from Member i as d_i and those allocated to him as a_i. The second-stage earnings of Members i are as follows.

$$\pi_i^2 = \begin{cases} -d_i + a_i & \text{if } i = 1, 2, 3 \\ 0 & \text{if } i = 4 \end{cases}$$

For treatments with *corruptibility*, Member 4 collects and allocates points among *all members* in her group. So Member 4 has a chance to appropriate points by reducing the earnings of other members. The second-stage earnings of Member i in this case are as follows.

$$\pi_i^2 = -d_i + a_i \quad \text{if } i = 1, 2, 3, 4$$

The second-stage earnings π_i^2 can be positive, zero or negative. If the second-stage earnings of Member i are negative, it means Member 4 imposes a centralized sanction upon Member i.

Decentralized punishment

Subjects in treatments with *no decentralized punishment* play a two-stage game. The earnings in a period equal to the sum of the earnings in the two stages. Subjects in treatments with *decentralized punishment* play a three-stage game. The earnings in a period equal to the sum of the earnings in the three stages.

In the third stage of treatments with *decentralized punishment*, all group members each decide how many points to deduct from the account of other members. The cost of a point deducted from the account of other member is 1/3. There is a budget constraint for decentralized punishment. The total number of points that a member can deduct from other members cannot exceed 3 $\times$ (his first-stage earnings + his second-stage earnings). Since subjects are guaranteed to leave the lab with positive earnings, allowing unlimited costly punishment is not possible. Denote the number of points deducted from the account of Member j by Member i as p_{ij}. The earnings of Member i in the third stage are in the following.

$$\pi_i^3 = -\sum_{j \neq i} p_{ji} - \sum_{j \neq i} p_{ij}/3 \ \text{ if } i = 1, 2, 3, 4$$

As stated before, group members at least keep 10 points in their private accounts after the second stage. This is to avoid a situation in which Member 4 in treatments with decentralized punishment takes away all the earnings of a member so that the member is not able to punish Member 4 afterwards.

Matching and information disclosure are as follows. Each treatment consists of 10 periods. First, at the beginning of each period, subjects are randomly (re)assigned to groups of 4 members. Then the four members in a group are randomly assigned to be Members 1, 2, 3, and 4. In other words, this experiment uses a strangers matching protocol. For public goods games, both designs of partners matching and strangers matching make sense in reality. Think about a corporation whose membership is stable and a temporary work team on a project. Previous studies also use both matching protocols. In particular, Nikiforakis (2008) compares both matching protocols and finds that even though contribution levels are lower with strangers matching than with partners matching, the counter-punishment effect on contributions is quite similar for both matching protocols. I choose the strangers matching protocol for clearer one-shot theoretical predictions.[6] Last, after each stage, group members are informed about all the decisions made in that stage and the corresponding earnings.

3.2.2 Hypotheses

First of all, I make standard subgame perfect equilibrium predictions in which individuals are assumed to only care for their own material payoffs. In the NoCorrupt/NoDP treatment, it does not hurt the centralized enforcer (Member 4) to implement sanctions. There exist multiple subgame perfect equilibria. One of the most efficient equilibria exists in which Members 1, 2, and 3 each contribute all 30 points and Member 4 punishes free riders such that $\pi_1^1 + \pi_1^2 = \pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2$. [7]

In the Corrupt/NoDP treatment, the dominant strategy for Member 4 is to take away as many points as possible for her own sake, leaving 10 points for each of Members 1, 2, and 3. Then there is no difference between that Members 1, 2, and 3 contribute nothing and that they contribute everything. So the most efficient equilibrium exists

[6] Fehr and Gächter (1999) show that regardless of whether there is decentralized punishment, the average contributions in public goods games between strangers matching and perfect strangers matching protocols are NOT significantly different. They point out that their strangers matching treatment represents a good approximation to true one-shot experiments.

[7] Here I identify a contributor as a free rider (cooperator) if he contributes fewer (more) points than the average contribution amount of the group.

in which Members 1, 2, and 3 each contribute all 30 points and Member 4 takes away others' points such that $\pi_1^1+\pi_1^2=\pi_2^1+\pi_2^2=\pi_3^1+\pi_3^2=10$ and $\pi_4^1+\pi_4^2=80+1.2\sum_{i=1}^{3}c_i$, where c_i is the contribution of Member i and $i=1,2,3$.

For treatments with decentralized punishment, the best response for all members in the third stage should always be not to punish any other member with a cost. In other words, the presence of decentralized punishment is hypothesized to have no impact on the enforcer's sanctions and group cooperation if individuals only care for their own material payoffs.

If I take into account preferences for fairness, for example, referring to the model by Fehr and Schmidt (1999), theoretical arguments and predictions can be different. In the NoCorrupt/NoDP treatment, not only is there no direct cost of Member 4's sanctions, but also there is no risk that she might be retaliated afterwards. Member 4 should be willing to sufficiently punish free riders so that a high contribution level will be induced. A cooperative subgame perfect equilibrium exists in which Members 1, 2, and 3 each contribute all 30 points (see *Proposition 1* in appendix B).

In the NoCorrupt/DP treatment, fairness preferences can motivate peer punishment in the third stage.[8] The possibility of retaliating Member 4 induces a cost for her sanctions. As suggested by Nikiforakis (2008) and Denant-Boemont *et al.* (2007), enforcers' fear for retaliation may decrease their sanctions on free riders. Since free-riding cannot be effectively prevented by centralized sanctions, a high contribution level cannot be sustained as easily as in the case in which retaliation threats are not present (see *Proposition 2* in appendix B). [9]

Hypotheses 1: effects of decentralized punishment possibility in the case with a non-corruptible enforcer

H1.1: The contribution level in the NoCorrupt/DP treatment is lower than that in the NoCorrupt/NoDP treatment.
H1.2: The intensity of centralized sanctions in the NoCorrupt/DP treatment is lower than that in the the NoCorrupt/NoDP treatment.

In the Corrupt/NoDP treatment, only if Member 4 is sufficiently averse to advantageous inequality in which she receives a higher payoff than other members do, she is willing to equally redistribute total earnings among all members such that $\pi_1^1+\pi_1^2=\pi_2^1+\pi_2^2=\pi_3^1+\pi_3^2=\pi_4^1+\pi_4^2=(110+1.2\sum_{i=1}^{3}c_i)/4$. In this case,

[8] In addition to the punishment driven by fairness preferences, there also proves to be a preference for retaliation, that is, the desire to hurt those who make one to suffer (Falk *et al.* 2005).

[9] On the other hand, the presence of decentralized punishment is likely to be a supplement to the decreased centralized sanctions, since it can also be targeted at free riders. In this sense, it is ambiguous whether contributions will decrease compared to the NoCorrupt/NoDP treatment.

the dominant strategy of other members is to contribute all 30 points (see *Proposition 3.1* in appendix B). Otherwise, out of self-interest, Member 4 will appropriate as much as possible, leading to a high level of (anti-social) centralized sanctions. Then for Members 1, 2, and 3, no matter how much they contribute, they will be left with only 10 points. The more they contribute, the more Member 4 can seize for herself. Due to their aversion to disadvantageous inequality in which they receive lower earnings than Member 4 does, the three members should not contribute anything. In this situation, there exists a unique uncooperative equilibrium in which Members 1, 2, and 3 contribute nothing and Member 4 takes away others' points such that $\pi_1^1 + \pi_1^2 = \pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = 10$ and $\pi_4^1 + \pi_4^2 = 80 + 1.2\sum_{i=1}^{3} c_i$ (see *Proposition 3.2* in appendix B).

In the Corrupt/DP treatment, the disadvantageous inequality aversion of other members will lead to intense punishments against Member 4 for her severe corruption. With a much larger cost, her excessive sanctions and corruptive behaviors may be restrained to some extent. To make a specific hypothesis about the change in contributions, I conduct an equilibrium analysis based on the model of fairness by Fehr and Schmidt (1999). By imposing a constraint upon disadvantageous inequality aversion, I present that the condition about the advantageous inequality aversion of Member 4 is more relaxed for an equilibrium with full cooperation to be sustained in the Corrupt/DP treatment than in the Corrupt/NoDP treatment (see *Proposition 4* in appendix B). So I hypothesize that the contribution level is more likely to be higher in the Corrupt/DP treatment than in the Corrupt/NoDP treatment.

Hypotheses 2: effects of decentralized punishment possibility in the case with a corruptible enforcer

H2.1: The contribution level in the Corrupt/DP treatment is higher than that in the Corrupt/NoDP treatment.
H2.2: The intensity of centralized sanctions in the Corrupt/DP treatment is lower than that in the Corrupt/NoDP treatment.

3.2.3 Procedure

The experiment was run in April and May, 2016 at Centerlab, Tilburg University and it was computerized using the Z-tree software (Fischbacher 2007). Subjects were Tilburg students and recruited via an online system. Upon arrival, subjects were assigned to computers by randomly choosing one card from a pile of numbered cards. After subjects were seated in the lab, printed copies of the instructions were distributed. The experimenter read instructions aloud in front of all subjects and then subjects answered control questions. After they answered all control questions correctly, the experiment started. When the experiment ended, earnings in one period were randomly chosen for subjects' final earnings.

In total, 14 sessions were run and 188 subjects participated in the experiment. The number of subjects was 52 in both the NoCorrupt/NoDP and Corrupt/NoDP treatment, 44 in the NoCorrupt/DP treatment, and 40 in the Corrupt/DP treatment. The number of subjects in each session is either 12 or 16. Each session consisted of one or two independent matching groups. On average each session lasted for one hour. Subjects earned 9.38 euro on average, with a minimum of 2.5 euro and a maximum of 33.5 euro.

3.3 Results

3.3.1 Contribution

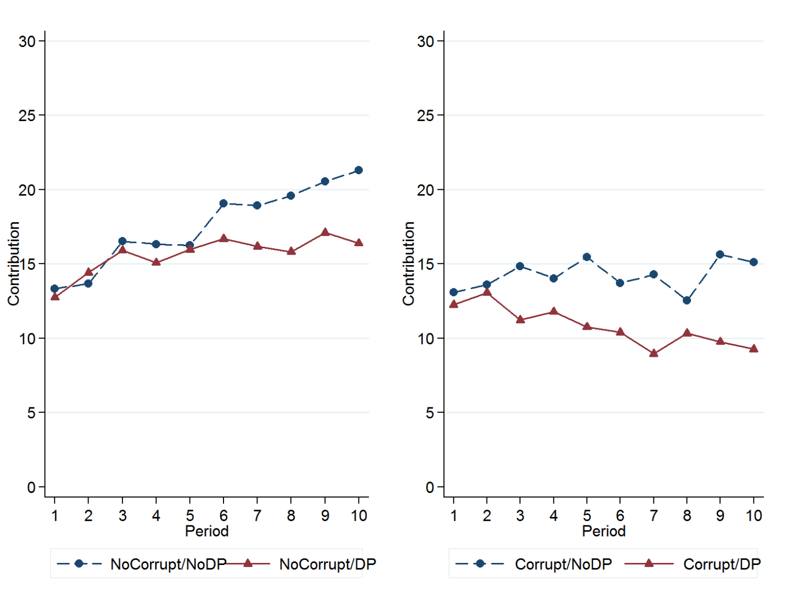


Figure 3.1: Evolutions of the average contributions over time

Figure 3.1 displays the evolution of the average contributions over time for all treatments. In line with previous experimental findings, the average contribution for each treatment starts at approximately 50% of subjects' endowments. The average contribution of the NoCorrupt/NoDP treatment mildly increases over time, from 13 to 21 points. The average contributions of the NoCorrupt/DP and Corrupt/NoDP treatments are relatively stable over time. The average contribution of the Corrupt/DP treatment declines over time, from 13 to 9 points. The average contribution in the

NoCorrupt/DP treatment lies below the average contribution in the NoCorrupt/NoDP treatment, and the average contribution in the Corrupt/DP treatment lies below the average contribution in the Corrupt/NoDP.

Table 3.2: Average contributions by treatment

Treatments	No corruptibility	Corruptibility
No decentralized punishment	17.00 (4.82) N=5	14.66 (3.13) N=5
Decentralized punishment	15.57 (2.94) N=5	10.90 (3.31) N=4
p-value	0.60	0.14

Notes: This table presents the average contributions per contributor by treatment. A unit of observation is an independent matching group. N is the number of independent matching groups. Standard deviations are in parentheses.

Table 3.2 displays the average contributions by treatment as well as rank-sum tests comparing across treatments. In the NoCorrupt/DP treatment, the average contribution level is 15.57, which is lower than that in the NoCorrupt/NoDP treatment (17.00). Also, the average contribution level in the Corrupt/DP treatment (10.90) is lower than that in the Corrupt/NoDP treatment (14.66).

I conduct rank-sum tests on contributions, using matching groups as units of independent observations. The contributions in the NoCorrupt/NoDP and NoCorrupt/DP treatments are not significantly different (p-value = 0.60). The same holds for the contributions in the Corrupt/NoDP and Corrupt/DP treatments (p-value = 0.14). Another finding is that the contribution level in the Corrupt/DP treatment is significantly lower than that in the NoCorrupt/DP treatment (p-value = 0.05; see appendix Table 3.10). It implies that corruptibility erodes cooperation when the enforcer can be punished.

I also parametrically test the effect of decentralized punishment possibility on contributions by controlling for period. Table 3.3 displays the parametric estimates. The column of “NoCorrupt” shows the results for treatments with no corruptibility and the column of “Corrupt” shows the results for treatments with corruptibility.

Table 3.3: Tobit random effects estimates of determinants of contributions

Variables	NoCorrupt Contribution	Corrupt Contribution
Dec punishment	-2.445	-4.460***
	(1.966)	(1.713)
Period	0.845***	-0.155
	(0.151)	(0.129)
Constant	14.940***	15.490***
	(1.489)	(1.429)
No. observations	720	690
No. subjects	96	92
Wald χ^2	38.32***	8.57**
Log likelihood	-2048.04	-2040.41

Notes: This table reports the estimates with a Tobit random effects model. The dependent variable is the individual-level contribution, which is left censored at 0 and right censored at 30. The variable Period captures the time trend. Dec punishment = 1 if the possibility of decentralized punishment is present; otherwise Dec punishment = 0. Robust standard errors (bootstrap) are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

The coefficient of Dec punishment is not significant for treatments with no corruptibility, while it is negative and significant for treatments with corruptibility. In the case with a non-corruptible enforcer, the presence of decentralized punishment does not significantly decrease contributions so that H1.1 is not supported. In the case with a corruptible enforcer, the presence of decentralized punishment reduces contributions substantially, which is at odds with H2.1 that contributions are increased by the presence of decentralized punishment.

One thing to notice is that the rank-sum test and parametric test lead to different results about whether there is a significant effect of the decentralized punishment possibility on contributions in the case with a corruptible enforcer. Since the statistical power is lower in the rank-sum test (62.2%) than in the parametric test ($\geq$ 99.95%), if there is truly an effect the parametric test is more likely to detect it.[10]

3.3.2 Centralized Sanction and Corruption

The next step is to examine whether the possibility of decentralized punishment weakens centralized sanctions. Members' second-stage earnings are determined by centralized sanctions. Since the total second-stage earnings of all group members are always equal to zero ($\sum_{i=1}^{4} \pi_i^2 = 0$), π_i^2 cannot be directly used to summarize the intensity of a centralized sanction. The intensity of a centralized sanction on Member i ($i = 1, 2, 3$)

[10] The statistical power is calculated with a two-tail test at 5% type I error level. The same argument also applies to the analysis in the next subsection.

is then measured by $max?(0, -\pi_i^2)$. If a member gets negative earnings in the second stage ($\pi_i^2 < 0$), the intensity of the centralized sanction on him is equal to $-\pi_i^2$, that is, the absolute value of his second-stage earnings; otherwise, the member is not sanctioned by the enforcer so that the intensity should be zero.

Table 3.4: Average intensities of centralized sanctions by treatment

Treatments	No corruptibility	Corruptibility
No decentralized punishment	3.49 (1.17) N=5	11.72 (4.85) N=5
Decentralized punishment	3.03 (1.23) N=5	10.61 (3.79) N=4
p-value	0.35	0.81

Notes: This table presents the average intensities of centralized sanctions on each contributor by treatment. A unit of observation is an independent matching group. N is the number of independent matching groups. Standard deviations are in parentheses.

Table 3.4 presents the average intensities of centralized sanctions on Members 1, 2, and 3 by treatment. In the NoCorrupt/DP treatment, the average intensity is 3.03, which is lower than that in the NoCorrupt/NoDP treatment (3.49). Also, the average intensity in the Corrupt/DP treatment (10.61) is lower than that in the Corrupt/NoDP treatment (11.72).

I conduct rank-sum tests on intensities of centralized sanctions, using matching groups as units of independent observations. The intensities of centralized sanctions in the NoCorrupt/NoDP and NoCorrupt/DP treatments are not significantly different (p-value = 0.35). The same holds for the comparison between the Corrupt/NoDP and Corrupt/DP treatments (p-value = 0.81). Another finding is that the presence of corruptibility always increases centralized sanctions substantially (p-value ≤ 0.01; see appendix Table 3.11). This result indicates that Member 4 is inclined to embezzle money through redistribution in case she gets a chance.

I also conduct Tobit random effects estimations to examine the effect of decentralized punishment possibility on centralized sanctions. There are two model specifications. In the first specification, in addition to Period and Dec punishment, I add Free-riding which denotes the extent to which a subject contributes fewer than the group average contribution. The second model specification is added with an interaction term between Dec punishment and Free-riding. Adding the interaction term is to explore to what extent free riding induces a weaker centralized sanction with the presence of decentralized punishment compared to the case in which decentralized punishment is not possible. Table 3.5 presents the results separately for treatments with no corruptibility (in "NoCorrupt" columns) and those with corruptibility (in "Corrupt" columns).

Table 3.5: Tobit random effects estimates of determinants of centralized sanctions

	Model 1		Mode 2	
	NoCorrupt	Corrupt	NoCorrupt	Corrupt
Variables	Cen sanctions	Cen sanctions	Cen sanctions	Cen sanctions
Dec punishment	-1.887***	-0.235	0.274	-0.921
	(0.715)	(1.514)	(1.109)	(1.522)
Free-riding	1.699***	1.492***	1.963***	1.403***
	(0.128)	(0.161)	(0.207)	(0.206)
Dec punishment × Free-riding			-0.548**	0.290
			(0.273)	(0.272)
Period	0.081	2.233***	0.103	2.252***
	(0.111)	(0.171)	(0.107)	(0.179)
Constant	-6.090***	-7.389***	-7.056***	-7.223***
	(0.932)	(1.675)	(0.876)	(1.540)
No. observations	720	690	720	690
No. Subjects	96	92	96	92
Wald χ^2	180.92***	213.38***	225.20***	241.27***
Log likelihood	-1250.67	-2047.15	-1244.88	-2046.7

Notes: This table presents the estimates with Tobit random effects models. The dependent variable is the intensity of a centralized sanction on Members 1, 2, and 3, which is left censored at zero. Free-riding = max (0, Group average contribution - contribution). Robust standard errors (bootstrap) are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

In the first model specification, the coefficient of Dec punishment is negative and significant for treatments with no corruptibility. It means that centralized sanctions imposed by a non-corruptible enforcer are weakened by the presence of decentralized punishment so that H1.2 is confirmed. But this significance does not hold for the case with a corruptible enforcer, which does not support H2.2 that the presence of decentralized punishment decreases centralized sanctions imposed by a corruptible enforcer.

The coefficient of Free-riding is positive and significant for all cases, which indicates Member 4 does punish a subject harder if the subject defects more. In the second model specification, the coefficient of the interaction term between Dec punishment and Free-riding is significant for treatments with no corruptibility. It means free-riding induces a weaker centralized sanction of a non-corruptible enforcer with the presence of decentralized punishment.

But is the weakening effect of decentralized punishment possibility strong enough to lead to a much severer free-riding problem? I look at when a subject contributes fewer than the group average contribution, whether the decrease in his second-stage earnings dominates the increase in his first-stage earnings. Suppose Members 1, 2, and 3 each contribute x. For each of them, the first-stage earnings are thus equal to $30 - x + 3x \times 0.55 = 30 + 0.65x$. If Member 1 contributes $x - 3$, then his first-stage earnings will increase by 1.35 points.

In the NoCorrupt/NoDP treatment, if the gap between a free rider's contribution and the group average contribution increases by one point, the free rider's second-stage earnings will decrease by 1.96 points.[11] Since now Member 1's contribution is lower than the group average contribution by 2 points, his second-stage earnings will decrease by $1.96 \times 2 = 3.92$ points. Thus his total earnings in this period decrease by $3.92 - 1.35 = 2.57$ points as a result of contributing fewer points. In other words, in this case the empirical best response for Members 1, 2, and 3 is not to free ride.

In the NoCorrupt/DP treatment, if the gap between a free rider's contribution and the group average contribution increases by one point, his second-stage earnings will decrease by $1.96 - 0.55 = 1.41$ points. Now if Member 1 contributes $x - 3$, his second earnings decrease by $1.41 \times 2 = 2.82$ points. Thus his earnings during the first two stages decrease by $2.82 - 1.35 = 1.47$ points. On the one hand, the presence of decentralized punishment decreases the effectiveness of centralized sanctions on free-riding with $1.47 < 2.57$. This drives the observation in Figure 3.1 that contributions are lower in the NoCorrupt/DP treatment than in the NoCorrupt/NoDP treatment. On the other hand, even though the centralized sanctions on free riders are weakened, the empirical best response is not to free ride either with $1.47 > 0$.

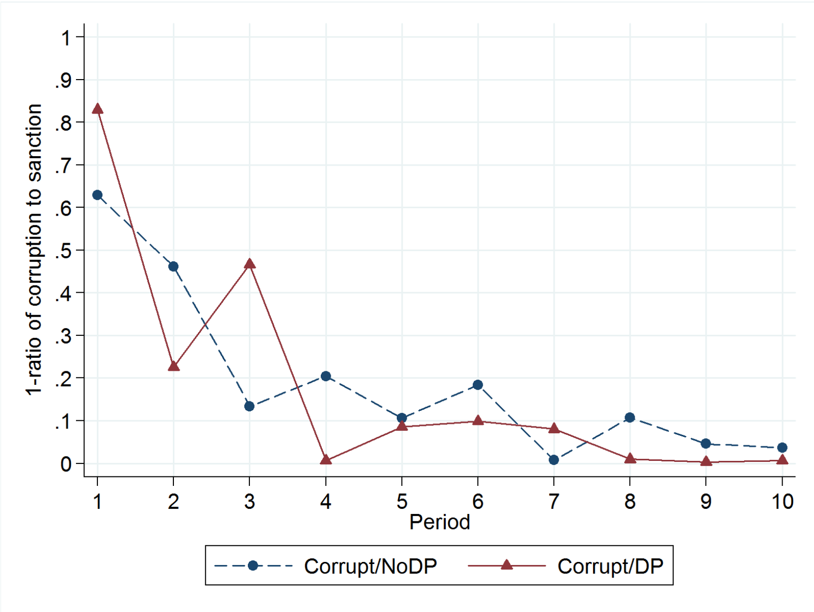
I then examine whether corruptive behaviors of Member 4 are different between the two treatments with corruptibility. To be comparable with a centralized sanction on per contributor, I divide the second-stage earnings of Member 4 by 3 to measure how many points Member 4 "steals" from each contributor on average. Table 3.6 shows that the average severities of corruption in the Corrupt/NoDP and the Corrupt/DP treatments are 10.03 and 9.77 respectively. There is no significant difference between the corruption severities in the two treatments (p-value = 1.00).

Table 3.6: Average severities of corruption by treatment

Treatments	Corruptibility
No decentralized punishment	10.03 (5.88) N=5
Decentralized punishment	9.77 (4.02) N=4

Notes: This table presents the average severities of corruption by treatment. A unit of observation is an independent matching group. N is the number of independent matching groups. Standard deviations are in parentheses.

[11] 1.96 is the marginal effect of free-riding on the uncensored latent variable. It can be viewed as the marginal effect of free-riding on the second-stage earnings considering how the intensity of centralized sanctions is measured.



Notes: This figure presents the ratio $\frac{centralized\ sanctions - corruption}{centralized\ sanctions}$ over periods by treatment with corruptibility, where corruption is measured by the average number of points that Member 4 "steals" from each other member and the intensity of centralized sanctions is equal to the number of points a member loses in Stage 2 (same definitions as before).

Figure 3.2: The ratio of redistributions among contributors to centralized sanctions

In treatments with corruptibility, there are two purposes for Member 4 to take points away from contributors, one being to increase her own earnings and the other being to redistribute earnings among contributors. Thus centralized sanctions can be divided into two parts: corruption and redistributions among contributors. Figure 3.2 shows that for both treatments, the ratios of redistributions to centralized sanctions decrease saliently over periods. It means corruptible enforcers become greedier and less responsible over time.

3.3.3 Decentralized Punishment

The hypotheses are based on the assumption about the retaliation incentive to "harm those who let one suffer" (Nikiforakis 2008). I test this assumption by exploring whether Members 1, 2, and 3 will deduct more points from the account of Member 4, if they receive severer centralized sanctions. Table 3.7 presents the estimation results.

Table 3.7: Tobit random effects estimates of determinants of punishment on Member 4

Variables	NoCorrupt/DP Punishment on Member 4	Corrupt/DP Punishment on Member 4
Cen sanctions	0.681***	0.951***
	(0.236)	(0.167)
Period	-0.492	-0.428
	(0.312)	(0.496)
Constant	-10.010**	-18.390***
	(3.909)	(4.447)
No. observations	330	300
No. subjects	44	40
Wald χ^2	9.66***	36.94***
Log likelihood	-412.51	-503.14

Notes: This table presents the estimates with a Tobit random effects model. The dependent variable is number of points deducted from the account of Member 4, which is left censored at zero. Cen sanctions stands for the intensity of the centralized sanction imposed upon the punisher. Robust standard errors (bootstrap) are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

The coefficients of Cen sanctions in both treatments are positive and significant. In the NoCorrupt/DP treatment, a one-point increase in the intensity of a centralized sanction on the punisher increases his punishment on Member 4 by 0.681 points. In the Corrupt/DP treatment, a one-point increase in the centralized sanction on the punisher increases his punishment on Member 4 by 0.951 points. The two results confirm that the level of decentralized punishment imposed upon the enforcer increases with the intensity of preceding centralized sanctions on the peer punisher.

Members 1, 2, and 3 also impose punishment upon each other. In the NoCorrupt/DP treatment, around 60% of the total punishments by them are imposed upon each other, and 2/3 of these punishments are targeted at free riders (see Table 3.12 in appendix). In the NoCorrupt/NoDP treatment, the average intensity of the centralized sanctions on free riders is 7.35 points, which is reduced to 5.71 with the presence of decentralized punishment.[12] In the NoCorrupt/DP treatment, the average size of the decentralized punishment on free riders by contributors is 4.30 points, which is strong enough to offset the decrease in pro-social centralized sanctions. This can help explain why contributions do not decrease significantly with the presence of decentralized punishment in the case with a non-corruptible enforcer, even though the intensity of centralized sanctions decreases.

In the Corrupt/DP treatment, around 80% of the punishments by Members 1, 2, and 3 are imposed on Member 4 and most of these punishments can be counted

[12]The average intensity is equal to the total intensity of centralized sanctions on free riders divided by the *number of free riders*. The same goes for the average size of the decentralized punishment on free riders.

as counter-punishment. This is consistent with the fact that the embezzlement and anti-social sanctions of Member 4 substantially impair other members' benefits and generate great inequality. Driven by both the retaliation incentive and the aversion to inequality, contributors impose severe punishment on Member 4.

3.3.4 Welfare

The previous experimental evidences are mixed about whether first-order or second-order decentralized punishment increases or decreases net earnings. Most studies find that decentralized punishment marginally increases or does not affect net earnings (e.g. Ostrom *et al.* 1992; Page *et al.* 2005; Nikiforakis 2008) and some find that decentralized punishment lowers net earnings (e.g. Fehr and Gächter 2000, 2002; Sefton *et al.* 2007).

So far I have shown that the presence of decentralized punishment is *not* helpful to cooperation in cases with a centralized enforcer. Combined with the fact that decentralized punishment induces a cost, it is not surprising to see that welfare is reduced by the presence of decentralized punishment. Table 3.8 presents the average period earnings by treatment.

Table 3.8: Average period earnings by treatment

Treatments	No corruptibility	Corruptibility	p-value	Row total
No decentralized punishment	42.80 (4.34) N=5	40.70 (2.82) N=5	0.25	41.75 (3.62) N = 10
Decentralized punishment	34.65 (4.87) N=5	30.79 (3.51) N=4	0.22	32.93 (4.54) N = 9
p-value	0.05	0.01		0.002
Column total	38.72 (6.11) N= 10	36.30 (6.00) N=9	0.34	37.57 (6.01) N=19

Notes: This table presents the average period earnings of each subject by treatment. A unit of observation is an independent matching group. N is the number of independent matching groups. Standard deviations are in parentheses.

The average period earnings across all the treatments are 37.57 points. In the NoCorrupt/DP treatment, the average period earnings are 34.65, which are lower than the earnings in the NoCorrupt/NoDP treatment (42.80). Also, the average period earnings are lower in the Corrupt/DP treatment (30.79) than in the Corrupt/NoDP treatment (40.70). And both differences are significant (p-value ≤ 0.05), indicating that the presence of (second-order) decentralized punishment erodes welfare in the social dilemma regulated by a centralized enforcer. Corruptibility also reduces welfare, but not as saliently as the possibility of decentralized punishment does.

3.4 Discussion

A question that has not been answered lies in the comparison between the two treatments with corruptibility, that is, what drives the decrease in contributions with the possibility of decentralized punishment. Ignoring complicated interactions between contributions, centralized sanctions, and decentralized punishment, I look at the correlation between contributions and period earnings of contributors. Table 3.9 presents the result.

Table 3.9: Linear random effects estimates for period earnings

Variables	Corrupt/NoDP Period earnings	Corrupt/DP Period earnings
Contribution	0.176**	0.071
	(0.084)	(0.151)
Period	-1.342***	-2.285***
	(0.246)	(0.254)
Constant	33.920***	34.930***
	(1.652)	(2.255)
No. observations	390	300
No. subjects	52	40
Wald χ^2	38.17***	36.94***

Notes: This table presents the estimates with a linear random effects model. The dependent variable is the period earnings for Members 1, 2, and 3. Robust standard errors (bootstrap) are in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

The coefficient of Contribution is significant and positive for the Corrupt/NoDP treatment but not for the Corrupt/DP treatment. I also pool the data of the two treatments and find that the coefficient of the interaction term between the treatment dummy and Contribution is significant (p-value < 0.001). In other words, high contributions are profitable for the subjects in the Corrupt/NoDP treatment, but not for those in the Corrupt/DP treatment. So subjects should contribute more in the Corrupt/NoDP treatment than in the Corrupt/DP treatment.

There can be two reasons for why the correlation between contributions and period earnings is different between the two treatments. One is that subjects in the Corrupt/DP treatment implement severe anti-social punishment on cooperators in the third stage. But in this treatment, the percentage of the decentralized punishment on cooperators is only 9%, which can hardly explain the decrease in contributions. The other possible reason is that the enforcer in the Corrupt/DP treatment implements less pro-social centralized sanctions compared to the Corrupt/NoDP treatment. I test the correlation between contributions and a contributor's total earnings during the first two stages for the Corrupt/DP treatment. The coefficient of Contribution is also insignificant, which indicates that the enforcer in the Corrupt/DP treatment does not redistribute earnings among contributors in a pro-social way.

Why does a corruptible enforcer implement less pro-social centralized sanctions with the possibility of decentralized punishment? In other words, why is a corruptible enforcer less responsible for improving cooperation with the possibility of getting punished? A responsibility-alleviation effect may be an explanation. Charness (2000) argues that shifting responsibility for an outcome to an external authority dampens internal impulses towards honesty, loyalty, or generosity. Charness *et al.* (2012) show that without bearing the duty of setting salaries, workers provide lower effort levels. There are some other related arguments. Barkema (1995) provides empirical evidence that executive performance is worse with close supervision. Griffith (1993) finds that unless monitoring is active, performance under physical monitoring is lower than with no monitoring.

In this study, a corruptible enforcer may initially have a sense of duty to improve cooperation. Even though the enforcer embezzles money, she acts in a relatively pro-social way, e.g. taking away fewer points from the accounts of cooperators than from the accounts of free riders. But when other members can informally monitor and regulate the corruptible enforcer, she might feel that her authority is challenged. She no longer cares about whether cooperators earn more points than free riders do, since she expects to be retaliated by all other members anyway due to her corruptive behaviors. In summary, with a limited and non-deterrent level, the possibility of punishing a corruptible enforcer may even worsen the situation by weakening her responsibility perception.

3.5 Conclusion

This paper uses experiments to study the effects of decentralized punishment possibility on centralized sanctions for both non-corruptible and corruptible enforcers as well as cooperation under their regulations. The results are mixed.

For a non-corruptible enforcer, her centralized sanctions are weakened by the possibility of decentralized punishment as predicted, but contributions do not decrease significantly. This result is in line with the outcome in Balafoutas *et al.* (2014). They study the effect of counter-punishment on the effectiveness of third-party punishment. Each third-party punisher can also be viewed as a non-corruptible centralized enforcer. They find that cooperation does not decrease substantially, even though the willingness of third-party punishers to sanction free riders is decreased by the possibility of counter-punishment.

The counter-punishment effect on the effectiveness of a non-corruptible centralized institution seems weaker than that on the effectiveness of decentralized punishment, since the latter effect proves to be significantly negative (e.g. Denant-Boemont *et* *al.* 2007; Nikiforakis 2008). It could be that the retaliation incentive for a peer's punishment is stronger than that for a non-corruptible enforcer's sanctions, because subjects may consider centralized sanctions to be more authorized and impartial.

For a corruptible enforcer, the intensity of centralized sanctions does not change, but the pro-sociality of her centralized sanctions is reduced by the possibility of retaliating the enforcer, which leads to lower contributions. This result is different from what is found by Van Leeuwen *et al.* (2015). They study the effect of ostracism on cooperativeness when there is a leader distributing the group output and find that cooperativeness is increased by the presence of ostracism.

There are two major differences between our designs which may drive the different result. One is that Van Leeuwen *et al.* (2015) use partners matching while I use s-trangers matching. The other difference is that in their study players can fully exclude the authority by majority voting, while in my study players can only limitedly reduce the payoff of the enforcer.

3.6 Appendix

3.6.A Instructions

A.1 Instructions for the Corrupt/DP treatment

Welcome to our experiment. Please follow the instructions carefully.

The experiment consists of 10 periods. At the end of the experiment you will be paid your earnings of one period, which will be randomly selected. In each period, your earnings depend on your decisions and the decisions of other participants. During the experiment your earnings are denoted in points and its exchange rate is 4 points = 1 Euro. You will receive your final earnings by bank transfer within three working days after the experiment.

At the beginning of each period, participants will be randomly (re)assigned to groups of 4 members. Then the four members in a group are randomly assigned to be Member 1, Member 2, Member 3, and Member 4. Members 1, 2 and 3 are each given an endowment of 30 points and Member 4 is given an endowment of 20 points.

Each period proceeds with three stages.

Stage 1

Members 1, 2 and 3 each decide how much of their endowment they want to contribute to the group account. Member 4 does not make a decision in this stage.

The return from the group account depends on the total amount of points that Members 1, 2 and 3 contribute. Specifically, every point a member contributes to the group account gives each group member a return of 0.55 points.

Hence, the earnings of group members in the first stage can be expressed as follows.

1st Stage Earnings (Members 1, 2 and 3) = 30 - (this member's contribution to the group account) + 0.55 * (total group contribution to the group account).

1st Stage Earnings (Member 4) = 20 + 0.55 * (total group contribution to the group account);

At the end of this stage, group members are informed how many points each member has contributed to the group account and about each member's 1st stage earnings.

Stage 2

Members 1, 2 and 3 do not make a decision in this stage.

Member 4 first chooses how many points to collect from all members in her or his group, and then decides how to allocate the collected points among all members in her or his group. Points collected from a member range between zero and (1st stage earnings of that member - 10). In other words, each member at least keeps 10 points in her or his account.

2nd stage earnings can be expressed as follows:

2nd Stage Earnings = - (points that are collected from this member by Member 4) + (points that are allocated to this member by Member 4);

Note that the total number of points collected from all group members by Member 4, should be equal to the total number of points that are allocated to all group members by Member 4.

At the end of this stage, group members are informed of Member 4's collection and allocation decisions, and the 2nd stage earnings of each member.

Stage 3

All group members each decide how many points to deduct from other members. Each point that you deduct from other members costs you 1/3 points. The total number of points that you deduct from other members cannot exceed 3 * (your 1st stage earnings + your 2nd stage earnings).

The earnings of group members in this stage can be expressed as follows.

3rd Stage Earnings = - (total points that are deducted from this member) - (1/3)*

(total points this member deducts from other members)

At the end of this stage, group members are informed of their 3rd stage deduction decisions and earnings.

Earnings of a Period

Earnings of a period = 1st Stage Earnings + 2nd Stage Earnings +3rd Stage Earnings

At the end of each period, group members are informed about their earnings of the current period.

Summary

- The experiment lasts for 10 periods.
- One period will be randomly chosen to determine your earnings.
- Your earnings in a period are determined by your own decisions and the decisions of other participants.
- At the beginning of each period, participants will be randomly (re)assigned to groups of 4 members. Then the four members in a group are randomly assigned to be Member 1, Member 2, Member 3, and Member 4.
- Each period consists of three stages.
- Earnings in the first stage are determined by the number of points Members 1, 2 and 3 contribute to the group account.
- Earnings in the second stage are determined by the points which Member 4 collects from, and allocates to, all members in her or his group.
- Earnings in the third stage are determined by the points that are deducted by and from group members.
- After each stage, group members are informed about all the decisions made in that stage and the corresponding earnings.
- Earnings of the period are the sum of the earnings in the three stages.

Procedure and Questions

After reading the instructions, you will be asked to answer a few control questions to check your understanding. The first period of the experiment will start as soon as all the participants have correctly answered the control questions. At the end of the experiment, please wait until your seat number is called and fill in payment details. Please be quiet during the entire experiment and do not talk to other participants. If you have a question, please raise your hand and you will be answered privately.

A.2 Summary of Instructions for the NoCorrupt/NoDP treatment

- The experiment lasts for 10 periods.
- One period will be randomly chosen to determine your earnings.
- Your earnings in a period are determined by your own decisions and the decisions of other participants.
- At the beginning of each period, participants will be randomly (re)assigned to groups of 4 members. Then the four members in a group are randomly assigned to be Member 1, Member 2, Member 3, and Member 4.
- Each period consists of two stages.
- Earnings in the first stage are determined by the number of points Members 1, 2 and 3 contribute to the group account.
- Earnings in the second stage are determined by the points which Member 4 collects from, and allocates to, Members 1, 2 and 3.
- After each stage, group members are informed about all the decisions made in that stage and the corresponding earnings.
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- Each period consists of three stages.
- Earnings in the first stage are determined by the number of points Members 1, 2 and 3 contribute to the group account.

- Earnings in the second stage are determined by the points which Member 4 collects from, and allocates to, Members 1, 2 and 3.
- Earnings in the third stage are determined by the points that are deducted by and from group members.
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- Earnings in the first stage are determined by the number of points Members 1, 2 and 3 contribute to the group account.
- Earnings in the second stage are determined by the points which Member 4 collects from, and allocates to, all members in her or his group.
- After each stage, group members are informed about all the decisions made in that stage and the corresponding earnings.
- Earnings of the period are the sum of the earnings in the two stages.

3.6.B An Equilibrium Analysis

The equilibrium analysis below is based on the utility model by Fehr and Schmidt (1999), which can be given by

$$U_i = \pi_i - \alpha_i \sum_{i \neq j} max\{\pi_j - \pi_i, 0\} - \beta_i \sum_{i \neq j} max\{\pi_i - \pi_j, 0\},$$
$$\alpha_i \geq 0, \beta_i \geq 0, \beta_i < \alpha_i.$$[13]

[13]In Fehr and Schmidt (1999), the utility is expressed as $U_i = \pi_i - \frac{\alpha_i}{n-1} \sum_{i \neq j} max?\{\pi_j - \pi_i, 0\} - \frac{\beta_i}{n-1} \sum_{i \neq j} max?\{\pi_i - \pi_j, 0\}$ and n is the number of group members. For the convenience of expression, the part of n in the present analysis is dismissed.

B.1 NoCorrupt/NoDP ($\pi_i = \pi_i^1 + \pi_i^2$)

Proposition 1: *If $\alpha_i \leq 1.325$, where $i = 1, 2, 3$, in the NoCorrupt/NoDP treatment there exists a cooperative subgame perfect Nash equilibrium as follows:*

1) In Stage 1, Members 1, 2, and 3 each contribute 30 points, that is, $c_1 = c_2 = c_3 = 30$.

2) In Stage 2,

a. if $c_1 = c_2 = c_3$, Member 4 does not do anything in the second stage such that $\pi_1^2 = \pi_2^2 = \pi_3^2 = 0$.

b. otherwise,

b.1. if $\sum_{i=1}^{3} c_i < 30$, Member 4 redistributes earnings such that $\pi_1 = \pi_2 = \pi_3 = (90 + 0.65\sum_{i=1}^{3} c_i)/3$;

b.2. if $30 \leq \sum_{i=1}^{3} c_i \leq 40/0.45$, Member 4 redistributes earnings such that $\pi_f = 50 - 0.45\sum_{i=1}^{3} c_i$ and $\pi_{i \neq f} = \pi_4 = 20 + 0.55\sum_{i=1}^{3} c_i$, where $i = f$ is the lowest contributor, that is, $c_f = min\{c_1, c_2, c_3\}$;

b.3. if $\sum_{i=1}^{3} c_i > 40/0.45$, Member 4 redistributes earnings such that $\pi_f = 10$ and $\pi_{i \neq f} = (80 + 0.65\sum_{i=1}^{3} c_i)/2$, where $i = f$ is the lowest contributor, that is, $c_f = min\{c_1, c_2, c_3\}$.

Proof

First, I show that a one-shot deviation from full contribution is NOT profitable in Stage 1.

Assume $c_2 = c_3 = 30$ so that $\sum_{i=1}^{3} c_i = 60 + c_1$.

i) If $c_1 = 30$, then $\pi_1 = \pi_2 = \pi_3 = 49.5$, $\pi_4 = 69.5$.

$$U_1 = 49.5 - \alpha_1(69.5 - 49.5) = 49.5 - 20\alpha_1.$$

ii) If Member 1 deviates to $c_1 \leq 40/0.45 - 60$, then $\pi_1 = 23 - 0.45c_1$, $\pi_2 = \pi_3 = \pi_4 = 53 + 0.55c_1$.

$$U_1 = 23 - 0.45c_1 - 3\alpha_1(53 + 0.55c_1 - 23 + 0.45c_1)$$

$$= 23 - 3\alpha_1 c_1 - 0.45c_1 - 90c_1 \leq 23.$$

The condition for $49.5 - 20\alpha_1 \geq 23$ is $\alpha_1 \leq 1.325$.[14]

iii) If Member 1 deviates to $30 > c_1 > 40/0.45 - 60$, then $\pi_1 = 10$, $\pi_2 = \pi_3 = (119 + 0.65c_1)/2$.

[14] Fehr and Schmidt (1999) suggest a simple discrete distribution of α_i and β_i based on large experimental evidence they have on the ultimatum game. According the suggested discrete distribution, it is very unlikely that α_i with groups of four members exceeds 1.3.

$$U_1 = 10 - \alpha_1(53 + 0.55c_1 - 10) - 2\alpha_1[(119 + 0.65c_1)/2 - 10]$$

$$= 10 - 142\alpha_1 - 1.1\alpha_1 c_1 \leq 10.$$

The condition for $49.5 - 20\alpha_1 \geq 10$ is $\alpha_1 \leq 1.975$.

Second, I argue that one-shot deviations from equilibrium strategies in Stage 2 are NOT profitable for Member 4 either.

In cases a. and b.1, equally redistributing earnings among Member 1, 2, and 3 does not hurt Member 4 anyway, since the inequality in earnings is not increased.

In cases b.2 and b.3, the average payoff of Members 1, 2, and 3 is lower than that of Member 4. So long as a redistributing strategy of Member 4 does not make any other's earnings higher than hers, the strategy won't be worse than an equal division (among Members 1, 2, and 3). The reason is that the advantageous inequality of Member 4 is the same as with an equal redistribution *and* the disadvantageous inequality she faces is zero. For b.3, it is easy to check that when $\sum_{i=1}^{3} c_1 > 40/0.45$, $\pi_{i \neq f} = (80 + 0.65\sum_{i=1}^{3} c_i)/2 < \pi_4 = 20 + 0.55\sum_{i=1}^{3} c_i$.

B.2 NoCorrupt/DP ($\pi_i = \pi_i^1 + \pi_i^2 + \pi_i^3$)

Proposition 2*: Considering the case in which $\alpha_i \geq 0.5$, in the NoCorrupt/DP treatment the strategy profile in the following cannot be sustained as a subgame perfect equilibrium:*

1) In Stages 1 and 2, the strategies are the same as displayed in Proposition 1.

2) In Stage 3, the members who earn the fewest points during the first two stages punish all other members such that the final earnings of all members are equal.

Proof

Notice that if no one is sufficiently concerned about disadvantageous inequality ($\alpha_i < 0.5$), it is always a best response for players not to punish in Stage 3. Then there is no difference between the cases with and without the decentralized punishment possibility. For a simple and more interesting discussion, here I only consider the case in which all members are sufficiently concerned about inequality ($\alpha_i > 0.5$). I check whether the strategy profile described in *Proposition 2* can be sustained as a subgame perfect equilibrium.

First, I show that a one-shot deviation from full contribution in Stage 1 is NOT profitable for Members 1, 2, and 3.

Assume $c_2 = c_3 = 30$ so that $\sum_{i=1}^{3} c_i = 60 + c_1$.

i) If $c_1 = 30$, then $\pi_1^1 + \pi_1^2 = \pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = 49.5$, $\pi_4^1 + \pi_4^2 = 69.5$.

Each of Members 1, 2, and 3 imposes punishment p_{i4} on Member 4 such that

$$\pi_1 = \pi_4 \implies 49.5 - p_{i4}/3 = 69.5 - 3p_{i4} \implies p_{i4} = 7.5 \implies U_1 = \pi_1 = 47.$$

ii) If Member 1 deviates to $c_1 \leq 40/0.45 - 60$, then $\pi_1^1 + \pi_1^2 = 23 - 0.45c_1$, $\pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = \pi_4^1 + \pi_4^2 = 53 + 0.55c_1$.

Then Member 1 punishes all three other members such that

$$\pi_1 = \pi_4 = 23 - 0.45c_1 - p_{14} = 53 + 0.55c_1 - p_{14}.$$

There is no such feasible punishment. At most, Member 1 can punish others until he uses up his total earnings during the first two stages. Thus his utility must be non-positive.

iii) If Member 1 deviates to $30 > c_1 > 40/0.45 - 60$, then $\pi_1^1 + \pi_1^2 = 10$.
Member 1 can only punish up to the budget constraint such that $p_{12} + p_{13} + p_{14} = 10$ and his final utility must be non-positive.

Second, a one-shot deviation from equally redistributing in Stage 2 on the normal path ($c_1 = c_2 = c_3 = 30$) is NOT profitable for Member 4 since the deviation won't reduce inequality or others' punishment on her in the third stage.

Third, I demonstrate that a one-shot deviation to no redistribution in Stage 2 on the punishment path ($c_1 < 30$) CAN be profitable for Member 4.

i) If $30 > c_1 > 40/0.45 - 60$, then $\pi_1^1 + \pi_1^2 = 10$, $\pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = (119 + 0.65c_1)/2$, $\pi_4^1 + \pi_4^2 = 53 + 0.55c_1$.

For simplicity, consider the case of $c_1 = 29$.

$$max(p_{14}) = 30 \implies min(\pi_4) = 38.95$$

$$\implies min(U_4) = 38.95(1 - \beta_4) - (119 + 0.65 \times 29 - 38.95 \times 2)\alpha_4$$

$$\implies min(U_4) = 38.95(1 - \beta_4) - 59.95\alpha_4.$$

ii) If Member 4 deviates to no redistribution in the case of $c_1 = 29$, then $\pi_1^1 + \pi_1^2 = 49.95$, $\pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = 48.95$.

Members 2 and 3 together punish Member 1 and 4 in Stage 3 with $p_{21} = p_{31}$ and $p_{24} = p_{34}$, such that

$$\pi_2 = \pi_3 = 48.95 - p_{24}/3 - p_{21}/3 = \pi_4 = 68.95 - 2p_{24} = \pi_1 = 49.95 - 2p_{21}$$

$$\implies p_{21} = p_{31} = 3.125, \quad p_{24} = p_{34} = 12.625$$

$$\implies U_4 = \pi_4 = 43.7 > 38.95 > 38.95(1 - \beta_4) - 59.95\alpha_4.$$

So a one-shot deviation to no redistribution in the second stage can be profitable for Member 4. This strategy profile cannot be sustained as a subgame perfect equilibrium in the NoCorrupt/DP treatment.

B.3 Corrupt/NoDP ($\pi_i = \pi_i^1 + \pi_i^2$)

Proposition 3.1*: If $\beta_4 \geq 0.25$, in the Corrupt/NoDP treatment a cooperative equilibrium can be sustained in which Members 1, 2, and 3 each contribute 30 points and Member 4 equally redistributes earnings among all members such that $\pi_i = (110 + 1.2\sum_{i=1}^{3} c_i)/4$.*

Proposition 3.2*: If $\beta_4 < 0.25$ and $\alpha_i > 0$, there exists a unique subgame equilibrium in which no one contributes any point and Member 4 always allocates as many points as possible to herself such that $\pi_4 = 80 + 1.2\sum_{i=1}^{3} c_i$.*

Proof

The equilibrium strategy for Member 4 is mainly dependent on β_4. Suppose the proportion of earnings that the enforcer keep for him/herself is k and each contributor gets one third of remaining earnings. And it should be that $k \geq (1-k)/3$ since $\alpha_4 > \beta_4$.

$$U_4 = \{k - \beta_4[k - (1-k)/3] \times 3\} \times \textstyle\sum_{i=1}^{4} \pi_i^1 = [(1 - 4\beta_4)k + \beta_4] \times \sum_{i=1}^{4} \pi_i^1.$$

If $(1 - 4\beta_4) \leq 0$, it is not profitable for Member 4 to allocate a larger share to herself. More specifically, if $\beta_4 \geq 0.25$, a cooperative equilibrium can be sustained that Members 1, 2, and 3 each contribute 30 points and Member 4 equally redistributes earnings among all members such that $\pi_i = (110 + 1.2\sum_{i=1}^{3} c_i)/4$.
If $\beta_4 < 0.25$, the equilibrium should be that Members 1, 2, and 3 each contribute zero and Member 4 always allocates as many points as possible to herself such that $\pi_4 = 110 + 1.2\sum_{i=1}^{3} c_i - 10 \times 3 = 80 + 1.2\sum_{i=1}^{3} c_i$, leaving Members 1, 2, and 3 each with 10 points.

B.4 Corrupt/DP ($\pi_i = \pi_i^1 + \pi_i^2 + \pi_i^3$)

Proposition 4: *Considering the case in which $\alpha_i \geq 0.5$, if $\beta_4 \geq 0.148$, in the Corrupt/DP treatment a cooperative equilibrium can be sustained in which*
1) In Stage 1, Members 1, 2, and 3 each contribute 30 points,
2) In Stage 2, Member 4 equally redistributes earnings among all members such that $\pi_i = (110 + 1.2\sum_{i=1}^{3} c_i)/4$,
3) In Stage 3, the members who earned the fewest points in the first two stages punishes all other members such that everyone make the same final earnings.

Proof

First, a one-shot deviation from full contribution in Stage 1 is NOT profitable, since Member 4 always equally redistribute earnings among all members so that $\pi_i = (110 + 1.2\sum_{i=1}^{3} c_i)/4$ is monotonically increasing with one's own contribution.

Second, I derive the condition in which a one-shot deviation from equally redistributing among all group members on the normal path ($c_1 = c_2 = c_3 = 30$) is NOT profitable for Member 4.

i) If Member 4 equally redistribute earnings among all members,

$$U_4 = \pi_4 = 54.5.$$

ii) If Member 4 deviates to no redistribution, so that

$$\pi_1^1 + \pi_1^2 = \pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = 49.5 \text{ and } \pi_4^1 + \pi_4^2 = 69.5.$$

Then in Stage 3, all three contributors punish Member 4 ($p_{14} = p_{24} = p_{34}$).

$$\pi_1 = \pi_4 \implies 49.5 - p_{14}/3 = 69.5 - 3p_{14} \implies p_{14} = 7.5 \implies U_4 = \pi_4 = 47 < 54.5.$$

iii) If Member 4 deviates to embezzlement, leaving each contributor with only 10 points, then we have

$$\pi_1^1 + \pi_1^2 = \pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = 10 \text{ and } \pi_4^1 + \pi_4^2 = 188$$
$$p_{14} = p_{24} = p_{34} = 30 \implies \pi_4 = 188 - 30 \times 3 = 98$$
$$U_4 = 98 - 3\beta_4 \times 98$$
$$98 - 3\beta_4 \times 98 \leq 54.5 \implies \beta_4 \geq 0.148.$$

In summary, only if $\beta_4 \geq 0.148$, a one-shot deviation from an equal redistribution to the severest embezzlement is NOT profitable for Member 4 on the normal path.

Third, I derive the condition in which a one-shot deviation from the strategy of an equal redistribution on a punishment path ($c_1 < 30$) is not profitable for Member 4.

i) If Member 4 equally redistributes earnings among all members,

$$U_4 = \pi_4 = (110 + 1.2\sum_{i=1}^{3} c_i)/4 = 45.5 + 0.3c_1.$$

ii) If Member 4 deviates to no redistribution,

$$\pi_1^1 + \pi_1^2 = 63 - 0.45c_1$$
$$\pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = 33 + 0.55c_1,\ \pi_4^1 + \pi_4^2 = 53 + 0.55c_1$$

.

In Stage 3, Members 2 and 3 together punish Members 1 and 4 ($p_{21} = p_{31}$, $p_{24} = p_{34}$)

$$33 + 0.55c_1 - (p_{21} + p_{24})/3 = 63 - 0.45c_1 - 2p_{21} = 53 + 0.55c_1 - 2p_{24}$$
$$\implies p_{24} = (130 - c_1)/4 \implies U_4 = \pi_4 = 20.5 + 0.8c_1$$
$$(45.5 + 0.3c_1) - (20.5 + 0.8c_1) = 25 - 0.5c_1 > 0$$

iii) If Member 4 deviates to embezzlement, leaving each contributor with only 10 points.

$$\pi_4^1 + \pi_4^2 = 80 + 1.2\sum_{i=1}^{3} c_i = 152 + 1.2c_1$$
$$\pi_1^1 + \pi_1^2 = \pi_2^1 + \pi_2^2 = \pi_3^1 + \pi_3^2 = 10$$

Then three contributors together punish Member 4 ($p_{14} = p_{24} = p_{34} = 30$)

$$\pi_4 = 152 + 1.2c_1 - 30 \times 3 = 62 + 1.2c_1$$
$$U_4 = (1 - 3\beta_4)(62 + 1.2c_1)$$
$$D = (45.5 + 0.3c_1) - (1 - 3\beta_4)(62 + 1.2c_1) = 186\beta_4 + (3.6\beta_4 - 0.9)c_1 - 16.5$$

The larger β_4 is, the more likely D is to be positive. Besides, when $3.6\beta_4 - 0.9 < 0$ ($\beta_4 < 0.25$), D decreases as c_1 increases so that D reaches the lowest level when $c_1 = 30$. I check as follows:
if $\beta_4 = 0.25$, then $D = 30 > 0$;
if $\beta_4 \geq 0.25$,then $D \geq 30 > 0$;
if $\beta_4 < 0.25$, then $D(c_1 = 30) = 294\beta_4 - 43.5 \geq 0 \implies$ if $0.25 > \beta_4 \geq 0.148$, then $D > 0$.

In summary, only if $\beta_4 \geq 0.148$, a one-shot deviation from an equal redistribution on a punishment path ($c_1 < 30$) is NOT profitable for Member 4.

So for this strategy profile to be sustained as a subgame perfect equilibrium in the Corrupt/DP treatment, we need $\beta_4 \geq 0.148$. In contrast, the condition is $\beta_4 \geq 0.25$ in the Corrupt/NoDP treatment. [15]

[15] Fehr and Schmidt (1999) suggest a simple discrete distribution in which $Prob(\beta_i = 0) = 30\%$, $Prob(\beta_i = 0.083) = 30\%$, and $Prob(\beta_i = 0.2) = 40\%$ (with groups of four members).

3.6.C Tables

Table 3.10: Average contributions by treatment

Treatments	No corruptibility	Corruptibility	p-value	Row total
No Decentralized Punishment	17.00 (4.82)	14.66 (3.13)		15.83 (4.02)
	N=5	N=5	0.25	N = 10
Decentralized punishment	15.57 (2.94)	10.90 (3.31)		13.49 (3.80)
	N=5	N=4	0.05	N = 9
p-value	0.6	0.14		0.29
Column total	16.29 (3.83)	12.99 (3.60)		14.72 (4.00)
	N= 10	N=9	0.06	N=19

Notes: This table presents average contributions per contributor by treatment with more results being reported. A unit of observation is an independent matching group. N is the number of independent matching groups. Standard deviations are in parentheses.

Table 3.11: Average intensities of centralized sanctions by treatment

Treatments	No corruptibility	Corruptibility	p-value	Row total
No decentralized punishment	3.49 (1.17)	11.72 (4.85)		7.60 (5.46)
	N=5	N=5	0.01	N = =10
Decentralized punishment	3.03 (1.23)	10.61 (3.79)		6.40 (4.70)
	N=5	N=4	0.01	N = 9
p-Value	0.35	0.81		0.51
Column total	3.26 (1.16)	11.22 (4.18)		7.03 (5.01)
	N= 10	N=9	0.00	N=19

Notes: This table presents average intensities of centralized sanctions on each contributor by treatment with more results being reported. A unit of observation is an independent matching group. N is the number of independent matching groups. Standard deviations are in parentheses.

Table 3.12: Decentralized punishment imposed by contributors

Treatment	Target of punishment	Type of punishment	Size of punishment
NoCorrupt/DP	Member 4	counter-punishment	428 (24.3%)
		on perverse sanctions	30 (1.7%)
		on omission	25 (1.4%)
		others	233 (13.2%)
	Members 1, 2, 3	on free riders	688 (39.0%)
		others	358 (20.4%)
Corrupt/DP	Member 4	counter-punishment	1341 (74.5%)
		on perverse sanctions	0 (0.0%)
		on omission	14.6 (0.8%)
		others	114.4 (6.4%)
	Members 1, 2, 3	on free riders	244 (13.6%)
		others	85 (4.7%)

Notes: This table reports the sizes of decentralized punishment by type of punishments. The sizes of decentralized punishment are measured by the numbers of points deducted from the accounts of subjects who are punished in the third stage. The corresponding percentages are in parentheses. Counter-punishment stands for the punishment upon Member 4 if *the punisher's second-stage earnings are negative.* Punishment on perverse sanctions denotes the punishment upon Member 4 if *the punisher's second-stage earnings are non-negative* and *the second-stage earnings of cooperator(s) are negative.* Punishment on omission denotes the punishment upon Member 4 if *the punisher's second-stage earnings are non-negative* and *the second-stage earnings of free rider(s) are non-negative.*

Chapter 4

The Effect of Interactions with Out-group Members on In-group-Out-group Differences: An Experimental Study

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4.1 Introduction

Groups are ubiquitous features of our social lives. A great challenge to organizations, communities, and societies is that people sometimes discriminate between in-group and out-group members. Attitudinal and perceptual gaps in favor of in-group members over out-group members, which are also called in-group-out-group differences, are well-documented by a number of laboratory studies (Tajfel *et al.* 1971; Brewer 1979; Bernhard *et al.* 2006; Goette *et al.* 2006; Charness *et al.* 2007; Chen and Li 2009; Sutter 2009; Currarini and Mengel 2016). This paper studies whether interactions with out-group members matter for in-group-out-group differences and whether the nature of these interactions matters for in-group-out-group differences.

It is not unusual to observe that people interact with out-group members in daily life. Suppose that a local competed with an immigrant for a job position. Will she view immigrants in a more hostile way? Or if she cooperated with an immigrant in a charity activity, will she treat immigrants more favorably? There are also a lot of examples in the field of sports, e.g. tennis players from different countries compete for a prize in a professional match. Do these competitions lead to more aggressive attitudes or behaviors toward their opponents' countries? Interaction with out-group members can even occur without direct and personal contacts. For instance, if two persons from different communities (know that they) both donated to a charity, will they feel closer to the people from the other community in general? Or if they bid for a precious CD on eBay, will they develop unpleasant feelings towards people from the other community. All these real-life examples inspire us to have a look at whether interactions with out-group members can matter for in-group-out-group differences.

Related to our study, there have been previous attempts to explore the effect of contact on the attitude toward a specific social group. On the one hand, positive contacts may reduce prejudice over out-groups. Van Oudenhoven *et al.* (1996) find that when the ethnic background of Turkish participants is made salient, the mean attitude of Dutch participants toward Turkish people is more favorable after they cooperated with Turkish participants on some tasks. Brown *et al.* (1999) show that after cooperative contacts with German participants, British participants view Germans more favorably in some positive dimensions, e.g. hardworking and efficient, provided that typical German participants were encountered. Adachi *et al.* (2016) demonstrate that after players played cooperatively with “suspicious” players from another university in violent video games, their out-group attitudes are improved.

On the other hand, negative contacts can lead to a more hostile attitude toward out-groups. Labianca *et al.* (1998) find that negative (interpersonal) relationships between employees from different departments of a company are positively correlated with higher perceived intergroup conflicts between departments. Brown *et al.* (2001) ask each British participant to describe a specific French person that he knows and to assess the nature of his contact with the French person in various dimensions, e.g. acquaintance, cooperative or conflictual, informal or formal, frequency. They find that if these contacts are rated as conflictual, participants on average exhibit more aggressive attitudes toward French.

We examine whether the effects on in-group-out-group differences of “personal” contact extend to an environment with “impersonal” interaction. We conduct a laboratory experiment in which subjects are randomly assigned to either a Red group or a Blue group and make distributional choices after having engaged in an interaction with an out-group member. We manipulate the nature of interactions with out-group members by imposing different payment structures for a real-effort task. In the competitive and cooperative treatments, subjects’ earnings for the task are calculated in competitive and cooperative ways respectively. We also include a baseline treatment in which earnings are calculated in a piece rate. After the real-effort task, each subject is asked to make a series of choices on allocations between herself and another subject, either from in-group or out-group (Chen and Li 2009; Currarini and Mengel 2016). The gaps between their choices when they are faced with in-group and out-group members can be identified as in-group-out-group differences. Our design allows us to examine how these differences vary with the nature of interactions.

Allport (1954) suggests that to be effective in reducing prejudice over out-groups, contact needs to be “personal”. Otherwise, people learn little about each other and intergroup friendships cannot occur. And according to a well-known meta study by Pettigrew and Tropp (2006) who include 713 independent samples from 515 studies, subjects in these studies at least get a chance to know with whom they interact or even have a real talk in the process of contact.[1] In our paper, an environment with

[1] Contacts range from temporary and structured conversations or activities in a lab setting, to sustained interventions (e.g. discussions on a specific topic or a joint trip) or encounters (e.g. interracial college roommates) in naturalistic environment.

"impersonal" interactions only imposes abstract (economic) interdependence between subjects. Besides, Allport (1954) points out that cooperation for a common goal is an important condition for contact to reduce prejudice saliently. Thus we in particular test whether the effect of the "impersonal" interaction is also positive in reducing in-group-out-group differences if the interaction involves cooperation for a common goal but not (or even is negative) if the interaction involves competition for a scarce resource.

Our paper is also related to studies on the effects of intergroup relations on in-group-out-group differences. For example, Bettencourt *et al.* (1998) let participants from two minimal groups work together on a task in either an intergroup competitive condition or an intergroup cooperative condition. Afterwards, participants are asked to rate each person's contribution and friendliness in the task. Their results indicate that competitive teams exhibit a higher level of in-group favoritism than cooperative teams do.[2] Goette *et al.* (2012) employ a two-stage game consisting a simultaneous prisoner's dilemma (PD) and then a third-party punishment game. In a condition of intergroup competition, a bonus is offered to each member from the group which got the highest total payoff in the PD. It is found that in-group favoritism in both cooperation and punishment is increased by intergroup competition.[3] In these studies, interactions are imposed directly between groups. In contrast, our paper focuses on interactions that occur between (two) persons from different groups. We examine whether the effect of interpersonal interactions can be generalized to group levels.

Our experimental results show that there is a difference in altruism between towards in-group and out-group members only when subjects receive higher earnings than their matched players do. Also, we demonstrate that cooperative interactions with out-group members decrease the in-group-out-group difference in altruism when subjects receive higher earnings than their matched players do, but competitive interactions do not affect it. When subjects receive lower earnings than their matched players do, their decisions are not affected by these interactions.

[2]Without a control group, they cannot tell which side (competitive or cooperative) drives the change in in-group-out-group differences. With a control group, our paper can make a conclusion about the driving source.

[3]In their study, the competitive condition increases the marginal benefit of favoring in-group members, even though the dominant strategy is to defect in both the competitive and baseline conditions. Our study separates the stage of interaction and the stage of measuring in-group-out-group differences, capturing a "spill-over" effect of interaction on in-group-out-group differences.

4.2 Experimental Design, Hypotheses and Procedure

4.2.1 Design

At the beginning of the experiment, the computer randomly assigns half of the subjects to the Red group and the other half to the Blue group. Red and Blue are meaningless labels. Subjects cannot communicate with each other. They won't know who will be in their own group or the other group. Also, subjects have no vested interest in serving their own group. We therefore use a "truly" minimal group paradigm.[4]

After the assignment of groups, there are three parts and a questionnaire. In part 1, subjects are asked to make a series of calculations within one minute. For each calculation, a subject sees four single-digit numbers. Two of them are in Red color and the other two in Blue color. The subject needs to sum up the two numbers in the same color as their group label. The computer quietly keeps track of the number of correct answers. The main purpose of the calculation task is to have efforts that generate earnings. The color of the numbers can serve as a priming tool to let subjects perceive their color-relevant group membership more saliently.

The earnings for this calculation task are then computed in different ways for different treatments. There are three treatments: neutral, cooperative, and competitive. In the neutral treatment, each subject earns 20 tokens for each correct answer she has, that is, she gets a piece-rate payment. In both the cooperative and competitive treatments, each subject is matched with an out-group member. For the cooperative treatment, the two subjects in a matched pair each earn 10 tokens for each correct answer based on the total number of correct answers they have. For the competitive treatment, a subject's earnings depend on whether she wins against the matched out-group member. The winner earns 40 tokens for each correct answer she has, while the loser earns nothing.[5]

There is a potential concern for implementing a cooperative payment structure based on the total number of correct answers. Subjects may not believe that the matched player makes as much effort as possible. In this sense, we cannot be sure that subjects perceive salient cooperativeness with the matched player. But other forms of a cooperative payment structure cannot perform better or theoretically even worse. For example, based on the minimum or maximum number of their correct answers,

[4]Since Tajfel *et al.* (1971), quite a few experimental studies on in-group-out-group differences have used the approach of assigning "minimal" groups by their preference of painting or some other ways which generate a mild sense of similarity with in-group members.

[5]In practice, we applied stochastic payment to the neutral and cooperative treatments, that is, only half of the subjects were randomly chosen by the computer to actually get paid. The rewards for each correct answer in the two treatments were then doubled compared to the original rewards. In this way, closer payoff distributions are generated for all treatments.

we cannot rule out the possibility that subjects shirk either. So long as the cost of making an effort is positive, under the "minimum" payoff structure there always exists an equilibrium in which both subjects shirk, and under the "maximum" payment structure there does not exist a symmetric equilibrium in which both subjects make every effort. In contrast, making every effort is the dominant strategy for subjects under the "total" payment structure, as long as making an effort is beneficial for the subject individually, that is, her personal cost of doing a calculation is lower than 20 tokens.

In part 2, each subject makes a number of choices on allocations of tokens between herself and another subject. In half of the experimental sessions, each subject is matched with a randomly selected subject from her own group (in-group matching); in the other half of the sessions, each subject is matched with a randomly selected subject from the other group (out-group matching). In the cooperative and competitive treatments, the member to whom each subject is matched now is different from the member to whom she was matched in part 1. We set 21 choice problems that each has an option X and an option Y. For each of these problems, subjects are asked to choose option X or option Y. Each option defines how many tokens they and their matched players will get if they choose that option.

For a specific measure of altruism, we refer to the model in Charness and Rabin (2002), which generalizes various social preferences with two parameters, ρ and σ. They capture to what extent a person cares for the other person's payoff when she gets a higher payoff and a lower payoff than the other person does respectively. In particular, to distinguish social preferences between over in-group and out-group members and across treatments, we construct $\rho_{g(j),t}$ and $\sigma_{g(j),t}$. $g(j)$ captures the group identity of subject j that subject i is matched with, that is, $g(j) \in \{in, out\}$; t stands for the treatment in which subject i plays, that is, $t \in \{N, CM, CO\}$. The utility is given as follows, where π_i and π_j are the payoffs of subjects i and j respectively.

$$u_i(\cdot|g(j), t) = \begin{cases} \rho_{g(j),t}\pi_j + (1 - \rho_{g(j),t})\pi_i & \text{if } \pi_i \geq \pi_j \\ \sigma_{g(j),t}\pi_j + (1 - \sigma_{g(j),t})\pi_i & \text{if } \pi_i < \pi_j \end{cases}$$

Based on the model, the 21 choice problems are presented in three scenarios. The first scenario of choice problems helps to calibrate σ and the second scenario helps to calibrate ρ. The third scenario induces further information for both ρ and σ. [6]

[6] Similar designs to elicit social preference parameters are employed in Blanco *et al.* (2011).

Table 4.1: Choice problems: scenario 1

	Option X		Option Y	
Problem	Your earnings	Other's earnings	Your earnings	Other's earnings
1	400	400	200	750
2	400	400	250	750
3	400	400	300	750
4	400	400	350	750
5	400	400	400	750
6	400	400	450	750
7	400	400	500	750

Table 4.2: Choice problems: scenario 2

	Option X		Option Y	
Problem	Your earnings	Other's earnings	Your earnings	Other's earnings
1	600	600	500	200
2	600	600	600	200
3	600	600	700	200
4	600	600	700	300
5	600	600	700	400
6	600	600	700	500
7	600	600	700	600

Table 4.3: Choice problems: scenario 3

	Option X		Option Y	
Problem	Your earnings	Other's earnings	Your earnings	Other's earnings
1	600	300	500	550
2	600	300	500	600
3	600	300	500	650
4	600	300	500	700
5	600	300	500	750
6	600	300	500	800
7	600	300	500	850

In part 3, each subject makes decisions for the same 21 choice problems as in part 2. If in part 2 she was matched with a subject from her own group, she is now matched with a subject from the other group, and vice versa. In this way, for measuring in-group-out-group differences, both between-subject and within-subject designs can be employed. Charness *et al.* (2012) suggest that between-subject analyses are more conservative, and that within-subject analyses are prone to carry-over and demand effects though they sometimes are a closer match to a theoretical perspective. In our case, the between-subject design is to obtain a cleaner measure of in-group-out-group differences. Also, it is interesting to see whether the within-subject design has an effect on subjects' decisions. On the one hand, the within-subject design is likely to increase subjects' awareness of out-group so that in-group-out-group differences may be stronger. On the other hand, in-group-out-group differences may not change (or even decrease) with the within-subject design, if subjects try to be consistent in two-part decision-making.

At the end of the experiment, results and earnings for the calculation task and choice problems are displayed. This can rule out the effect of outcomes of competitive and cooperative interactions, since we are not interested in it in the present study.

4.2.2 Hypotheses

Contact theory suggests that contact featured with cooperation for a common goal can reduce prejudice over out-groups. A number of experimental and empirical studies on contact and out-group attitudes also confirm that positive contacts bring about more favorable out-group attitudes. For negative contact, some papers show that conflictual contacts with out-group members lead to more aggressive or hostile out-group attitudes (see introduction).

In our study, on the one hand, the setting of "impersonal" interactions may reduce the chance that subjects develop friendships; on the other hand, uncontrolled negative feelings are less likely to occur between persons who interact with each other. These facts imply that "impersonal" interactions may not be intense enough to reproduce the effects of contact. We thus make weak hypotheses that cooperative interactions with out-group members decrease in-group-out-group differences while competitive interactions increase it or these interactions do not have an effect on in-group-out-group differences.

For testing the effects of interactions on in-group-out-group differences, we need to compare the differences in ρ and σ between the case in which subjects are matched with an in-group member and the case in which subjects are matched with an out-group member across treatments. The hypotheses are formalized as follows.

Hypotheses 1: the effect of competitive interactions on in-group-out-group differences
H1.a: $\rho_{in,N}\ (\sigma_{in,N}) - \rho_{out,N}\ (\sigma_{out,N}) = \rho_{in,CM}\ (\sigma_{in,CM}) - \rho_{out,CM}\ (\sigma_{out,CM})$
H1.b: $\rho_{in,N}\ (\sigma_{in,N}) - \rho_{out,N}\ (\sigma_{out,N}) < \rho_{in,CM}\ (\sigma_{in,CM}) - \rho_{out,CM}\ (\sigma_{out,CM})$

Hypotheses 2: the effect of cooperative interactions on in-group-out-group differences
H2.a: $\rho_{in,N}\ (\sigma_{in,N}) - \rho_{out,N}\ (\sigma_{out,N}) = \rho_{in,CO}\ (\sigma_{in,CO}) - \rho_{out,CO}\ (\sigma_{out,CO})$
H2.b: $\rho_{in,N}\ (\sigma_{in,N}) - \rho_{out,N}\ (\sigma_{out,N}) > \rho_{in,CO}\ (\sigma_{in,CO}) - \rho_{out,CO}\ (\sigma_{out,CO})$

H1.b means that competitive interactions with out-group members increase in-group-out-group differences. H2.b means that cooperative interactions with out-group members decrease in-group-out-group differences.

4.2.3 Procedure

The experiment was run in May, 2017 at Centerlab, Tilburg University and it was computerized using the Z-tree software (Fischbacher 2007). Subjects were Tilburg students and recruited via an online system. Upon arrival, subjects were assigned to computers by randomly choosing one card from a pile of numbered cards. Upon subjects were seated in the lab, printed copies of general instructions and instructions for part 1 were distributed. After they finished reading instructions, the experiment started. Subjects received instructions for part 2 and part 3 right after the preceding part had ended.

After subjects finished all the three parts, they filled in a questionnaire, including questions about age, gender, major of study, level of education, experience in experiments, race, and guesses for the numbers of their own and average correct answers in part 1. The guesses can be viewed as a proxy of the subjects' belief of how much they earned in part 1, which may have an income effect on their subsequent decisions in parts 2 and 3.

In total, 12 sessions were run and 160 subjects participated in the experiment. The number of subjects in each session is either 12 or 16. The numbers of subjects by treatment are displayed in Table 4.4. Each session lasted for 20-30 minutes. Subjects earned 9.28 euro on average, with a minimum of 4.06 euro and a maximum of 16.81 euro.

Table 4.4: Numbers of subjects by treatment

Treatment	In-out	Out-in
Cooperative	28	24
Neutral	24	28
Competitive	32	24

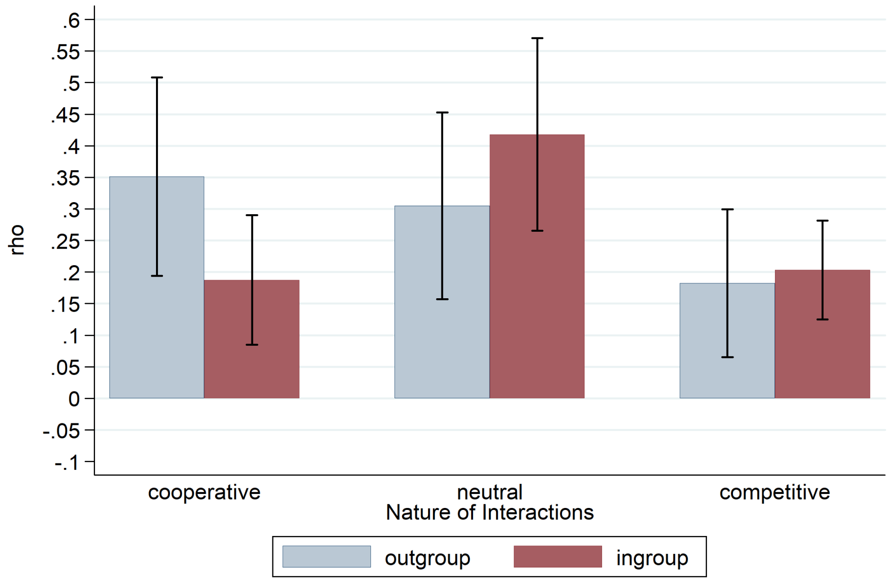
Notes: The table presents the numbers of subjects by treatment. The second (third) column reports the numbers for the sessions in which subjects are matched with in-group (out-group) members in part 2 and with out-group (in-group) members in part 3.

4.3 Results

To get a clean result of in-group-out-group differences as well as treatment effects, we first only focus on the decisions subjects made in part 2. In the subsection about the within-subject design, we take into account the decisions subjects made in part 3.

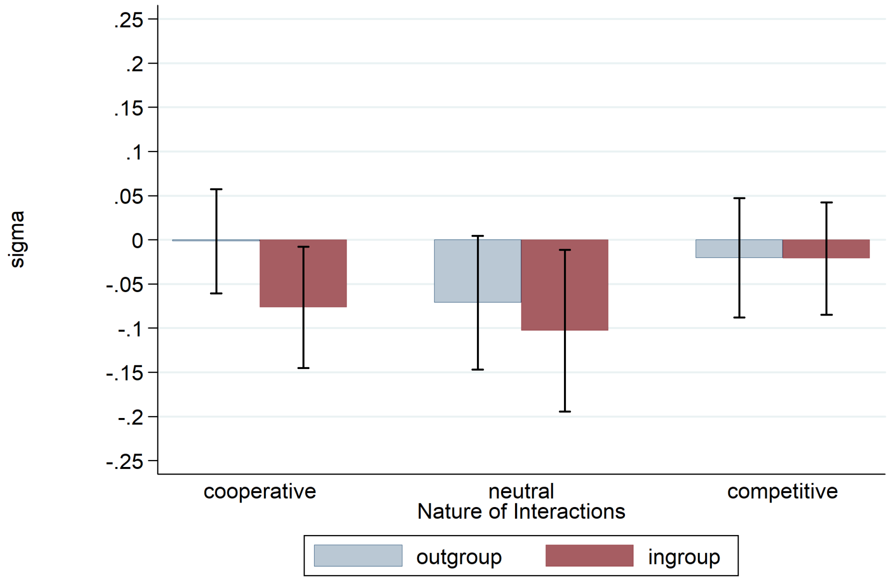
4.3.1 Effects of Group Identity on Social Preferences across Treatments

Let's first look at the descriptive statistics of ρ and σ. To calibrate ρ and σ manually, we use the observations of single-switch points in scenarios 2 and 1 respectively (see appendix A for details of calibrations). In part 2, 4 out of 160 subjects did not make choices that follow the single-switch rule in scenario 1 or 2. We therefore rule out the corresponding choices (5 observations) from this analysis. Figures 4.1 and 4.2 display the average calibrated ρ and σ by matching and treatment respectively.



Notes: The bars stand for calibrated ρ by matching and treatment (the nature of interactions). The spikes with caps stand for the corresponding 95% confidence intervals.

Figure 4.1: ρ by matching and treatment



Notes: The bars stand for calibrated σ by matching and treatment. The spikes with caps stand for the corresponding 95% confidence intervals.

Figure 4.2: σ by matching and treatment

For calibrated ρ, we can see that in the neutral treatment, its value is higher for matched in-group members than for matched out-group members, though the difference is not salient. In the competitive treatment, subjects marginally favor in-group members as well. In the cooperative treatment, we find that the favor over in-group members is reversed in the sense that subjects exhibit more altruism towards out-group members than towards in-group members.

For calibrated σ, we do not find salient in-group-out-group differences in both the neutral and competitive treatments. In the cooperative treatment, subjects exhibit milder negative attitude towards out-group members than towards in-group members.

Table 4.5: Average ρ and σ by matching and treatment

Parameter	Treatment	In-group	Out-group	p-value
ρ	Cooperative	0.19 (0.27) N = 28	0.35 (0.37) N = 24	0.13
	Neutral	0.42 (0.36) N = 24	0.31 (0.37) N = 26	0.29
	Competitive	0.20 (0.21) N = 31	0.18 (0.28) N = 24	0.58
σ	Cooperative	-0.08 (0.17) N = 27	-0.002 (0.14) N = 24	0.07
	Neutral	-0.10 (0.22) N = 24	-0.07 (0.19) N = 28	0.38
	Competitive	-0.02 (0.17) N = 31	-0.02 (0.16) N = 24	0.67

Notes: The table mainly presents the average values of ρ and σ by matching and treatment. Standard deviations are reported in the parentheses. N stands for the number of independent observations, that is, the number of subjects who made single-switch choices. The fourth column presents the p-values of the rank-sum tests comparing between in-group and out-group matching.

Table 4.5 displays the average values of calibrated ρ and σ by matching and treatment as well as rank-sum tests comparing between in-group matching and out-group matching for all treatments. In the neutral treatment, the average ρ for the in-group matching is 0.42, which is higher than that for the out-group matching (0.31). We conduct rank-sum tests, using subjects as units of independent observations. The difference in ρ between the in-group and out-group matching is not significant (p-value = 0.29).[7]

Similarly, in the competitive treatment, the average ρ is higher for the in-group matching (0.20) than for the out-group matching (0.18), but again the difference is not significant (p-value = 0.58).

In the cooperative treatment, the average ρ is higher for the out-group matching (0.35) than for the in-group matching (0.19). We see that cooperative interactions with out-group members change the difference in favor of in-group members to that in favor of out-group members.

Similarly, in both the neutral and competitive treatments we do not find significant differences in σ between in-group and out-group matching. In the cooperative treatment, subjects treat out-group matched players (-0.002) nicer than in-group matched

[7]The statistical power of the test is only 44.1% in a one-tail test at 5% type I error level. It may be too conservative to conclude that in-group-out-group differences do not exist in the neutral treatment. The same applies to the rank-sum test on σ.

players (-0.08), and the difference is marginally significant (p-value = 0.07).

4.3.2 Effects of Interactions on In-group-Out-group Differences

From the results above, we can see that the gap in calibrated ρ and σ for in-group and out-group members is different between the neutral treatment and the cooperative treatment. It implies that cooperative interactions with out-group members matter for in-group-out-group differences.

We employ parametric analyses to examine the treatment effects of interactions. Specifically, we use maximum simulated likelihood estimations and a logit specification with random individual effects.[8] Thus the probabilities of option X and Y being chosen can be expressed as below.

$$Prob(X) = \frac{e^{\gamma u(X)}}{e^{\gamma u(X)} + e^{\gamma u(Y)}}$$
$$Prob(Y) = \frac{e^{\gamma u(Y)}}{e^{\gamma u(X)} + e^{\gamma u(Y)}}$$

Take option X as an example. Suppose option X allocates x_i to subject i and x_j to subject j who is matched with subject i. $u_i(X|g(j), t)$ can be given by

$$u_i(X|g(j), t) = \begin{cases} \rho_{g(j),t} x_j + (1 - \rho_{g(j),t}) x_i & \text{if } x_i \geq x_j \\ \sigma_{g(j),t} x_j + (1 - \sigma_{g(j),t}) x_i & \text{if } x_i < x_j, \end{cases}$$

where $g(j)$ captures the group identity of subject j and t stands for the treatment (nature of interactions). $\rho_{g(j),t}$ and $\sigma_{g(j),t}$ are assumed to be linear functions of the exogenous variables below. In-group is the dummy variable for whether the subject is matched with an in-group member (= 1) or an out-group member (= 0). Competitive (Cooperative) is the dummy variable for whether the subject interacted with an out-group member in a competitive (cooperative) way (= 1) or not (= 0).[9] The treatment effects of competitive and cooperative interactions on in-group-out-group differences are captured by the coefficients of In-group × Competitive and In-group × Cooperative, respectively.

γ captures the sensitivity of subjects' decisions to $u(Y) - u(X)$. If γ is equal to zero, this model is equivalent to a random choice model with equal probability; when γ is infinitely large, the probability of choosing either option with higher utility approaches to one (McFadden 1981).

[8] The individual effects are added in the utilities of choosing option Y.

[9] So if a subject is involved in the neutral treatment, Cooperative = 0 and Competitive = 0.

Table 4.6: Maximum simulated likelihood estimates of determinants of ρ and σ

Variables	(1)	(2)
ρ		
In-group	0.044*	0.054**
	(0.023)	(0.023)
In-group × Competitive	-0.016	-0.035
	(0.032)	(0.032)
In-group × Cooperative	-0.089***	-0.109***
	(0.033)	(0.034)
Competitive	-0.056**	-0.052**
	(0.023)	(0.024)
Cooperative	-0.029	-0.040*
	(0.023)	(0.024)
Constant	0.203***	0.324***
	(0.016)	(0.070)
Controls	No	Yes
σ		
In-group	-0.012	-0.015
	(0.037)	(0.037)
In-group × Competitive	0.005	0.015
	(0.051)	(0.051)
In-group × Cooperative	-0.075	-0.030
	(0.054)	(0.054)
Competitive	0.041	0.015
	(0.037)	(0.038)
Cooperative	0.058	0.018
	(0.037)	(0.038)
Constant	-0.039	0.383***
	(0.026)	(0.114)
Controls	No	Yes
γ		
Constant	0.030***	0.031***
	(0.001)	(0.001)
Individual Effect		
Std.	33.225***	29.525***
	(2.993)	(3.087)
Observations	3360	3360
Log-likelihood	-1354.05	-1310.34

Notes: Controls stand for the characteristics of the subject, including age, gender, level of education, experiences in experiments, major of study, race, and confidence which is measured by the difference between the guesses for the numbers of one's own correct answers and the average correct answers in the calculation task. Column (1) displays the estimates when characteristics of subjects are not controlled for. Column (2) displays the estimates when characteristics of subjects are controlled for. Standard errors are in parentheses. ***p <0.01, **p <0.05, *p <0.1

Table 4.6 presents the results. Let's first look at the estimates for ρ. In the column (1), the constant term for ρ is positive and significant, which indicates that subjects in the *neutral* treatment on average exhibit altruism toward their matched *out-group* players when they receive higher earnings. Also, the coefficient of In-group is positive and significant (at 5% level in a one-tail test). It means that when subjects in the neutral treatment receive higher earnings than their matched players do, they on average exhibit a 22% (= 0.044/0.203) increase in altruism towards in-group matched players than towards out-group matched players.

For the treatment effects on in-group-out-group differences, we find that the coefficient of Competitive is negative and significant, but the coefficient of In-group ×

Competitive is not significant. It implies that competitive interactions decrease altruism (of the richer) towards both in-group and out-group members to the same extent. Thus H1.a cannot be rejected. The coefficient of In-group × Cooperative is negative and significant. It indicates that cooperative interactions with out-group members saliently decrease in-group-out-group differences in altruism, so that H2.a is rejected and H2.b is supported. And the decrease in the in-group-out-group difference is sharp enough to reverse in-group favoritism to out-group favoritism ($0.044 - 0.089 < 0$), which is consistent with what was observed in Figure 4.1. We also compare the coefficients of In-group × Competitive and In-group × Cooperative and the difference is significant (p-value = 0.03). It indicates that the nature of interactions does matter for in-group-out-group differences in altruism.

Next, let's look at the estimates for σ. None of the major variables' coefficients is significant. So when subjects receive lower earnings than their matched players do, their decisions are not significantly affected by these interactions in any sense.[10]

In summary, cooperative interactions with out-group members decrease the in-group-out-group difference when subjects earn more than their matched players do, while competitive interactions do not affect it. Subjects' altruism is not responsive to these interactions in any sense when they receive lower earnings than their matched players do.

4.3.3 Within-subject Design and Order Effect

Last, we take into account the observations in part 3 and study whether in-group-out-group differences will change with within-subject analyses and whether there is an order effect for the within-subject designs. Here we only consider the neutral treatment, ruling out potential interaction effects between sequential decision-making and treatment effects. In particular, for within-subject analyses of in-group-out-group differences, we separately look at the sessions in which subjects are first matched with in-group members and the sessions in which subjects are first matched with out-group members. Table 4.7 reports ρ and σ with within-subject and between-subject analyses for the neutral treatment.

[10] The statistical power is no lower than 99.95% for all the estimates (comparing to zero) in Table 4.6, with both two-tail and one-tail tests at 5% type I error level. It means if there is truly an effect of some variable, the parametric estimation in Table 4.6 is very likely to detect it.

Table 4.7: Within-subject and between-subject analyses for the neutral treatment

Variables		Within-subject In-out	Within-subject Out-in	Between-subject Part 2
ρ				
	In-group	0.02	0.044*	0.049*
		(0.026)	(0.023)	(0.026)
	Constant	0.218***	0.194***	0.192***
		(0.019)	(0.017)	(0.018)
σ				
	In-group	-0.013	0.050*	-0.013
		(0.034)	(0.029)	(0.044)
	Constant	-0.037	-0.039	-0.047
		(0.030)	(0.027)	(0.031)
γ				
	Constant	0.025***	0.026***	0.026***
		(0.002)	(0.002)	(0.002)
Individual Effect				
	Std.	53.680***	50.158***	46.537***
		(8.069)	(9.853)	(7.121)
Observations		1008	1176	1092
Log-likelihood		-439.18	-531.857	-480.7

Notes: The table reports the maximum simulated likelihood estimates with a similar random effect logit model to Table 4.6. The only difference is that now ρ and σ are assumed to be only dependent on Ingroup. The second column presents the estimates of the neutral treatment for subjects who are first matched with an in-group member; the third column presents the estimates of the neutral treatment for subjects who are first matched with an out-group member; the fourth column presents the estimates of the neutral treatment with part 2 observations alone. Standard errors are in parentheses. ***p <0.01, **p <0.05, *p <0.1

First, we compare the coefficients of In-group between the second (third) column and the fourth column, for both ρ and σ. Under the z-test which is suggested by Clogg *et al.* (1995), we find that none of these differences is significant.[11] It is indicated that the within-subject design induces the same level of in-group-out-group differences as the between-subject design does. This is consistent with the result from Chen and Li (2009) who show that there is no significant difference in both "group-differentiating" behaviors and self-reported attachment between within-subject and between-subject designs.

Second, we compare the coefficients of In-group between the second column and the third column. The coefficient of In-group is always smaller for the case in which subjects are first matched with in-group members than for the case in which subjects are first matched with out-group members. The difference is only marginally significant for σ (at 10% level with a one-tail z-test) and not significant for ρ.[12] We won't make too much out of the (at best) weak order effect. A tentative explanation is that when subjects are first matched with out-group members, they may be a bit more aware that this will be a game relevant to group identity, since it seems a bit more usual to

[11] $Z_\rho(column2, column4) = (0.020-0.049)/\sqrt(0.026^2+0.026^2) = -0.789$, $Z_\rho(column3, column4) = (0.044-0.049)/\sqrt(0.023^2+0.026^2) = -0.144$, $Z_\sigma(column2, column4) = 0$, $Z_\sigma(column3, column4) = (0.050+0.013)/\sqrt(0.029^2+0.044^2) = 1.196$.

[12] $Z_\rho(column2, column3) = (0.020-0.049)/\sqrt(0.026^2+0.023^2) = -0.691$, $Z_\sigma(column2, column3) = (0.050+0.013)/\sqrt(0.029^2+0.034^2) = 1.410 > 1.282$.

interact with in-group members.

4.4 Conclusion

This paper uses a minimal-group paradigm to study whether cooperative and competitive interactions with out-group members matter for in-group-out-group differences. We first confirm the existence of an in-group-out-group difference in altruism. We show that when subjects receive higher earnings than their matched players do, they exhibit more altruism toward an in-group matched player than toward an out-group matched player. Chen and Li (2009) find a 47% increase in ρ and a 93% increase in σ on average when subjects are matched with an in-group member compared to the case in which they are matched with an out-group member. We find a much weaker evidence that only ρ increases by 22% on average with in-group matching. But considering the fact that Chen and Li (2009) use the second mover's data in sequential games for the estimates of in-group-out-group differences in distributional preferences, our study captures in-group-out-group differences in "purer" altruism with data from dictator games.[13]

More importantly, we find that cooperative interactions with out-group members decrease in-group-out-group differences in altruism when subjects receive higher earnings than their matched players do, while competitive interactions do not affect it. This suggests that "impersonal" interaction which involves cooperation for a common goal can also reduce prejudice over out-groups. So as to weaken the difference favoring in-group members over out-group members, it is not necessary to create chances for people from different social groups to meet face-to-face or have a real talk. Linking them with cooperative economic interdependence is sufficient for a reduction in in-group-out-group differences. Besides, compared to Goette *et al.,* (2012) who find that intergroup competition increases in-group-out-group differences, our study suggests that competitive interactions between persons have a much weaker effect on in-group-out-group differences than intergroup competition does.

An interesting and unanswered question is that why subjects are unresponsive to any variation in the condition when they are at a disadvantage of payoff comparisons. Further studies on other moderators of in-group-out-group differences can be conducted. We are interested in whether this unresponsiveness also occurs to "poorer" people in cases with other moderators, which might shed a light on a study on in-group-out-group differences and poverty.

[13] Currarini and Mengel (2016) also capture in-group-out-group differences in altruism with dictator games. But since they do not distinguish between the altruism of the richer and the poorer, their outcomes are not very comparable with ours.

4.5 Appendix

4.5.A Single-switch Points and Calibrations of Parameters

Table 4.8: Single-switch points in scenario 2 and calibrations of ρ

Single-switch point in scenario 2	Range	Calibration
All for option Y	$\rho \leq -0.33$	-0.33
2	$-0.33 \leq \rho < 0$	-0.165
3	$0 \leq \rho < 0.2$	0.1
4	$0.2 \leq \rho < 0.25$	0.225
5	$0.25 \leq \rho < 0.33$	0.29
6	$0.33 \leq \rho < 0.5$	0.415
7	$0.5 \leq \rho < 1$	0.75
All for option X	$\rho \geq 1$	1

Notes: This table presents how calibrations of ρ are dependent on observed single-switch points *from option X to option Y* in scenario 2. For example, the sixth row displays if a subject's choice switches from option X to option Y at problem 5, the range of her ρ is $0.25 \leq \rho < 0.33$ and the calibration of ρ is 0.29. The range is derived from $u(X_{problem\ 4}) = 600 \geq u(Y_{problem\ 4}) = 300\rho + 700(1-\rho)$ and $u(X_{problem\ 5}) = 600 < u(Y_{problem\ 5}) = 400\rho + 700(1-\rho)$. The calibration 0.29 is the midpoint of the range, that is, $(0.25 + 0.33)/2 = 0.29$. For cases in which all choices are option Y (X), the calibration is obtained conservatively, that is, the upper-bound (lower-bound) of the range.

Table 4.9: Single-switch points in scenario 1 and calibrations of σ

Single-switch point in scenario 1	Range	Calibration
All for option Y	$\sigma \geq 0.36$	0.36
2	$0.3 \leq \sigma < 0.36$	0.33
3	$0.22 \leq \sigma < 0.3$	0.26
4	$0.125 \leq \sigma < 0.22$	0.173
5	$0 \leq \sigma < 0.125$	0.063
6	$-0.17 \leq \sigma < 0$	-0.085
7	$-0.4 \leq \sigma < -0.17$	-0.285
All for option X	$\sigma \leq -0.4$	-0.4

Notes: This table presents how calibrations of σ are dependent on observed single-switch points *from option X to option Y* in scenario 1. For example, the seventh row displays if a subject's choice switches from option X to option Y at problem 6, the range of her σ is $-0.17 \leq \sigma < 0$ and the calibration of σ is -0.085. The range is derived from $u(X_{problem\ 5}) = 400 > u(Y_{problem\ 5}) = 750\sigma + 400(1-\sigma)$ and $u(X_{problem\ 6}) = 400 \leq u(Y_{problem\ 6}) = 750\sigma + 450(1-\sigma)$. The calibration -0.085 is the midpoint of the range, that is, (-0.17+0)/2 = -0.085. For cases in which all choices are option Y (X), the calibration is obtained conservatively, that is, the lower-bound (upper-bound) of the range.

4.5.B Instructions

Sample Instructions (for Red group)

General

Welcome to the experiment. Please follow the instructions carefully.

Please be quiet during the entire experiment and do not talk to other participants. If you have a question, raise your hand. An experimenter will then come to your computer to answer the question privately.

The experiment consists of three parts and an additional questionnaire. You will receive instructions for Part 2 and Part 3 right after the preceding part has ended.

Your earnings depend on your decisions and performance, the decisions and performance of other participants, and chance. In all parts of the experiment your earnings are expressed in tokens. The exchange rate is 160 tokens = 1 Euro.

At the end of the experiment, you will be informed about the outcomes of all parts and about how much you earned in each part. Please follow the instructions of the experimenter then, and wait until your seat number is called before leaving the lab. You will receive your final earnings by bank transfer within three working days after the experiment.

Assignment to a group

At the beginning of the experiment, participants will be divided into two groups: a **Red** group and a **Blue** group. The computer will randomly assign half of the participants to the **Red** group and the other half to the **Blue** group. The group to which you are assigned will be shown on the computer screen before Part 1 starts. In all parts, all participants get the same tasks and earnings are calculated in the same way.

Part 1

[Intergroup Neutral Treatment]

Task

You are asked to make a series of calculations during one minute. For each calculation, you will see four single-digit numbers. Two of them are in **Red** color and the other two in **Blue** color. You are asked to sum up the two numbers in **Red** color. The computer quietly keeps track of the number of correct answers.

Example:

3 0 7 2

Your answer: ______

Submit

If and only if you type in 10 and click the “Submit” button, your answer counts as correct.

You can see the time left in seconds at the upper right-hand corner of the screen.

Earnings

Your earnings are calculated as follows:

Earnings = 40 tokens × your number of correct answers.

At the end of the experiment, half of the participants will be randomly chosen by the computer to actually get paid their earnings for this task. Also, at the end of the experiment, you will be informed about your number of correct answers and your earnings in Part 1.

[Intergroup Competition Treatment]

Task

You are asked to make a series of calculations during one minute. For each calculation, you will see four single-digit numbers. Two of them are in **Red** color and the other two in **Blue** color. You are asked to sum up the two numbers in **Red** color. The computer quietly keeps track of the number of correct answers.

Example:

3 0 7 2

Your answer: ______

Submit

If and only if you type in 10 and click the "Submit" button, your answer counts as correct.

You can see the time left in seconds at the upper right-hand corner of the screen.

Earnings

You will be matched with a participant from the **Blue** Group, and your earnings depend on whether you win against this participant. You win if you have more correct answers than the participant from the **Blue** group, and you lose if you have fewer correct answers than the participant from the **Blue** group. If both of you have the same number of correct answers, the winner will be determined randomly.

If you lose, your earnings are equal to 0 tokens. If you win, your earnings are calculated as follows:

Earnings = 40 tokens × number of correct answers.

You will be informed about your and the matched participant's numbers of correct answers and your earnings in Part 1 at the end of the experiment.

[Intergroup Cooperation Treatment]

Task

You are asked to make a series of calculations during one minute. For each calculation, you will see four single-digit numbers. Two of them are in **Red** color and the other two in **Blue** color. You are asked to sum up the two numbers in **Red** color. The computer quietly keeps track of the number of correct answers.

Example:

3 0 7 2

Your answer: ______

Submit

If and only if you type in 10 and click the "Submit" button, your answer counts as correct.

You can see the time left in seconds at the upper right-hand corner of the screen.

Earnings

You will be matched with a participant from the **Blue** group. Your earnings are calculated as follows:

Earnings = 20 tokens × total number of correct answers by you and participant from the **Blue** Group.

At the end of the experiment, half of the participants will be randomly chosen by the computer to actually get paid their earnings for this task. Also, at the end of the experiment, you will be informed about your and the matched participant's number of correct calculations and your earnings in Part 1 at the end of the experiment.

Part 2 [Ingroup Matching]

Task

You are asked to make a number of choices on allocations of tokens between yourself and another randomly selected participant of the **Red** group. That is, you will get to see 21 choice problems as follows, spread over three screens, that each has an option X and an option Y. For each of these problems, you are asked to choose option X or option Y. Each option defines how many tokens you and the other participant from the **Red** Group will get if you choose that option.

Table 1

	Option X		Option Y		Your Decision
Problem	Your earnings	Other's earnings	Your earnings	Other's earnings	
1	400	400	200	750	
2	400	400	250	750	
3	400	400	300	750	
4	400	400	350	750	
5	400	400	400	750	
6	400	400	450	750	
7	400	400	500	750	

Table 2

	Option X		Option Y		Your Decision
Problem	Your earnings	Other's earnings	Your earnings	Other's earnings	
1	600	600	500	200	
2	600	600	600	200	
3	600	600	700	200	
4	600	600	700	300	
5	600	600	700	400	
6	600	600	700	500	
7	600	600	700	600	

Table 3

	Option X		Option Y		Your Decision
Problem	Your earnings	Other's earnings	Your earnings	Other's earnings	
1	600	300	500	550	
2	600	300	500	600	
3	600	300	500	650	
4	600	300	500	700	
5	600	300	500	750	
6	600	300	500	800	
7	600	300	500	850	

For example, for Table 1 Problem 1, you and the matched participant both get 400 tokens if you choose option X, and you get 200 tokens and the matched participant gets 750 tokens if you choose option Y.

The other participant performs the same task.

Earnings

At the end of the experiment, the computer randomly picks one choice problem that counts for payment and randomly determines whether your decision or the matched participant's decision counts for payment.

You will be informed about the results and your earnings in Part 2 at the end of the experiment.

Part 3

In Part 3 the task and calculation of earnings are exactly the same as in Part 2 except that now the other participant is a randomly selected participant of the **Blue** group.

[Notice that the participant to whom you are matched now is different from the participant to whom you were matched in Part 1.]

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